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Ser. 2
Vol. 13
no 8

SER. II, VOL. 13 *June, 1951* No. 8

TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

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TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 13, No. 8, June, 1951.
Entered as second-class matter December 12, 1947, at the post office at Baltimore, Maryland, under the act of August 24, 1912. This publication is distributed to members and is published monthly from November to June, inclusive, by The New York Academy of Sciences, Mt. Royal and Guilford Aves., Baltimore 2, Md. Editorial office: 2 East Sixty-third Street, New York 21, N. Y.

Executive Director: EUNICE THOMAS MINER

A SURVEY OF STUDIES OF THE REACTION BETWEEN PERMANGANATE AND OXALATE IONS*

By RICHARD M. NOYES†

Permanganate ion oxidizes oxalate in acid solution with the formation of manganous ion and carbon dioxide. Frequently, the first encounter of a prospective chemist with this reaction is during a pencil and paper exercise on the balancing of equations for chemical reactions. By the time he has come to the answer,

$$2MnO_4^- + 5C_2O_4^= + 16H^+ \rightarrow 2Mn^{++} + 10CO_2 + 8H_2O,$$

he has often decided that he wants no further contact with the reaction. A year or two later, however, during his course in quantitative analysis, he may revise this unfavorable opinion. He finds that he must observe certain precautions, such as keeping the solution hot, but the reaction is a convenient way to standardize permanganate solutions, and the end point is so sharp that it is really a pleasant titration.

Finally, if our aspiring chemist examines the details of this reaction from the vantage point of a course in physical chemistry, he may well revert to his original impression that it is perverse and generally unpleasant. This perversity is illustrated by the observations of some representative solutions reported in TABLE 1. A comparison of solutions I and II shows that the reaction is greatly slowed by increasing the concentration of oxalate, an effect opposite to that normally anticipated. (Other experiments have demonstrated that the inhibition observed in these solutions is indeed due to the increase in concentration of oxalate rather than to the decrease in concentration of free hydrogen ion.) A comparison of solutions I and III illustrates the still more striking fact that manganous ion, one of the products of the reaction, has a very profound accelerating effect on the initial stages. This effect is so profound that, if there is no deliberate initial addition of manganous ion, several minutes may elapse before there is any evidence whatsoever that a chemical reaction is taking place.

It is a fundamental tenet of scientific investigation that such apparently anomalous results can be accounted for if sufficient information is available, and a great many investigators have examined this interesting reaction. Apparently, the first rate study was in 1866,[1] and we have found no less than twenty-nine subsequent publications[2–30] devoted to the mechanism of this one reaction. It was apparent from the outset that, when manganous ion is initially present in excess, the reaction proceeds in two stages. In the first stage, the color of permanganate is rapidly discharged, and the solution becomes yellow if the concentration of free oxalate ion is low and "cherry red" if the concentration of free oxalate ion is high. In the second stage, the reaction proceeds slowly to completion.

The second stage is the simpler one and is much better understood. In addition to the excess manganous and oxalate ions, the solution during this stage contains oxalate complexes of manganese (III) which decompose by a first order process. The equations for the decomposition of the mono-oxalato complex might be formulated

$$Mn(C_2O_4)^+ \rightarrow Mn^{++} + C_2O_4^=$$
$$Mn(C_2O_4)^+ + C_2O_4^= \rightarrow Mn^{++} + C_2O_4^= + 2CO_2.$$

The first step is presumed to be rate-determining, and the second step to be very fast.

* This paper, illustrated with lantern slides, was presented at the meeting of the Section on May 1, 1951.

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Some of these complexes were characterized spectrophotometrically by Cartledge and Ericks,[21] who showed that the yellow color observed in solutions I and III of TABLE 1 is due to $Mn(C_2O_4)_2^-$, and the reddish color observed in solution II is due to $Mn(C_2O_4)_3^{=}$. The definitive work on

TABLE 1

BEHAVIOR OF REPRESENTATIVE SOLUTIONS

Concentrations, moles/liter

Solution	*I*	*II*	*III*	*IV*
$HClO_4$	0.100	0.100	0.100	0.100
$Na_2C_2O_4$	.0050	.150	.0050	—
$MnSO_4$	.0050	.0050	—	0.0050

Observations

The following effects were observed when the above solutions were made 0.00050 M in $KMnO_4$:

I. The color of permanganate faded with formation of a yellow solution which faded further. The last tinge of permanganate color disappeared after about 30 seconds, and the yellow solution became water-white by the end of 5 minutes.

II. The solution immediately assumed a-reddish color quite distinct from that of permanganate. This color faded slowly, becoming faint after 4 hours. A slight residual coloration remained even after several days of standing in contact with air.

III. There was no initial change, but the color began to fade at 10 minutes with the formation of a yellow solution as in I. The last trace of permanganate color disappeared just after 14 minutes, and the solution became water-white by 17 minutes.

IV. There was no initial change. A distinct turbidity appeared at 3 minutes-20 seconds, and the precipitate was heavy by 5 minutes. The permanganate color seemed gone by 9 minutes.

the decomposition of these complexes is due to Taube,[29] who obtained kinetic evidence for the existence of $Mn(C_2O_4)^+$ and showed that the rate of oxidation of oxalate during the second stage of the reaction could be described by an equation of the form

$$-\frac{d[Mn(III)]}{dt} = 2k_1[Mn(C_2O_4)^+] + 2k_2[Mn(C_2O_4)_2^-] + 2k_3[Mn(C_2O_4)^{=}].$$

Taube reports that at 25° C. $k_1 = 11.8$ min^{-1}, $k_2 = 0.046$ min^{-1}, and $k_3 = 0.0205$ min^{-1}. We see that those complexes of manganese (III) having the most oxalate exhibit the least tendency to undergo the decomposition reaction to manganese (II) and partially-oxidized oxalate. Therefore, the work of Taube provides a satisfactory explanation for the unusual inhibition of the rate by excess oxalate ion.

The first stage of the reaction is more complex, and the data presented in TABLE 1 suggest that this stage is greatly accelerated by excess manganous ion. If the excess is sufficiently large, the first stage goes rapidly to completion to form a solution of oxalate complexes of manganese (III) whose oxidizing titer (as measured by adding excess iodide ion and titrating with thiosulfate) is not much less than that of the original permanganate. If no manganous ion is added to the solution, the rate of the initial reaction is very slow but increases autocatalytically until all of the permanganate has been reduced to manganese (III).

These observations indicate that permanganate does not react with oxalate directly but that in the first stage the permanganate oxidizes manganous ion with the formation of manganese (III). In order to explain this initial fast reaction, Launer[15] postulated the rapid equilibrium

$$MnO_4^- + 4Mn^{++} + 8H^+ \rightleftarrows 5Mn^{+++} + 4H_2O.$$

According to Launer, the presence of oxalate ion shifted this equilibrium to the right by the formation of the complexes of manganese (III) discussed above.

Three objections can be raised to the Launer postulate that permanganate and manganous ion are in rapid equilibrium which is shifted by oxalate:

(1) The first objection was raised by Polissar[22] on the basis of his observation, recently confirmed by Adamson,[23] that the exchange reaction between permanganate and manganous ions is negligibly slow compared to the times involved in the reaction with oxalate. Therefore, any equilibria between permanganate and manganous ions must involve at least two intermediate valence states of manganese which do not exchange rapidly with each other.

(2) An acid solution of permanganate

and manganous ions is thermodynamically unstable with respect to solid manganese dioxide, yet the mixture may remain for several minutes before any precipitate starts to form (see solution IV in TABLE 1). Once the reaction starts, it is catalyzed by the surface of the manganese dioxide. If there is a rapid equilibrium among the oxidation states of manganese, the slow step in the precipitation of manganese dioxide must involve hydrolysis of a manganese (IV) ion or be a supersaturation effect. If a solution of manganese (III) in strong acid is diluted, however, it immediately precipitates manganese dioxide by a disproportionation reaction.[33] These observations, while not conclusive, argue against an equilibrium mechanism.

(3) In solution I of TABLE 1, the mixture contained an excess of both manganous and oxalate ions. Nevertheless, the color of permanganate was not discharged for a period of twenty to thirty seconds. This observation provides the most direct indication that the reaction between permanganate and manganous ions in the presence of oxalate is not a rapid equilibrium at low concentrations.

Because of these uncertainties in previous interpretations, Dr. Janet M. Malcolm at Columbia University has recently investigated the initial stages of the reaction in the presence of very low concentrations of manganous ion. These studies, which will be reported elsewhere, substantiate the idea that, in the first stage of the reaction, oxalate ion catalyzes the oxidation of manganous ion by permanganate rather than shifting an already established equilibrium.

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SECTION OF GEOLOGY AND MINERALOLOGY

LITHOLOGIC AND FLORAL FACIES IN THE PARÍCUTIN ASH DEPOSITS, MEXICO*

By ERLING DORF†

Terrestrial pyroclastic deposits of the type associated with Parícutin Volcano are believed to be rather unusual compared to the more normal nonvolcanic clastic deposits of either present or past. As sedimentary units, the Parícutin deposits do not seem to "follow the rules," so to speak, which can be applied to most sedimentary formations.

Among the unusual qualities of the Parícutin deposits, observed during February and March, 1945, the following were noted as the more striking:

(1) The pyroclastic debris is strictly juvenile, of immediate magmatic source. Practically all of the debris, from the coarse volcanic rubble to the finest ash, is composed of volcanic lava or its exploded equivalents, with essentially no admixture of accidental, foreign materials. In this respect, the deposits are obviously more closely akin to igneous than to the common clastic rocks.

(2) The area in which the initial pyroclastics are deposited is extremely local in extent, being confined to a roughly elliptical area less than 10 miles in diameter. Among normal clastic sedimentary deposits, only a few, such as alluvial fan deposits and local lacustrine deposits, are of comparable localization.

(3) The pyroclastics are laid down as a mantle deposit, forming a veneer of variable thickness upon an irregular, preexisting topography. In this respect, the rare aeolian mantles, such as loess deposits, are among the few comparable types of other clastic deposits.

(4) The pyroclastic mantle is deposited with extreme rapidity, building up a sedimentary blanket as much as scores of feet in maximum thickness within a few years. Comparable rapidity of deposition is rare among the normal processes of deposition of other clastic sediments.

(5) Within the localized area of deposition, the pyroclastics show a greater textural range than is usually found in so limited an area in most other sedimentary deposits. Radially outward from the vent agglomerates, there are coarse to fine cinder deposits, very coarse volcanic rubble deposits, interbedded with coarse to fine ash and lava flows, then lateral gradation to medium to fine ash deposits, which may also be interbedded with lava flows.

(6) Primary sedimentary features in the pyroclastic deposits show an unusual diversity. Bedding, for example, may vary from a normal horizontal attitude to a dip as high as 30 degrees within a few yards. Local unconformities, often produced during and following a single rainy spell, are of frequent occurrence within the ash deposits. Cross-bedding, channel-deposits, irregular lenses, slump and fan deposits are commonly associated within short distances with more regular, even stratification of the ash.

(7) Buried plant remains are very abundant in the ash deposits, whereas the remains of both invertebrates and vertebrates are rare. This fact apparently is the result of the environment's quite suddenly becoming unfavorable to practically the total invertebrate and vertebrate population. Much of the plant population, on the other hand, is able to survive at least temporarily, except where buried by lava or excessively thick pyroclastics, and continues to supply leaves, needles, cones, and other plant materials to the record buried by the ash deposits.

In pointing out the above unusual features of terrestrial pyroclastic deposits, it is believed that a number of ancient volcanic units, such as the Yellowstone Absaroka volcanics and the Livingston vol-

* This paper, illustrated with lantern slides, was presented at the meeting of the Section on May 7, 1951.

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canics, exhibit many of the characteristics here discussed. An understanding of the processes involved in the deposition of the pyroclastics of the Parícutin area should be of considerable help in the interpretation of the depositional history in these and other ancient volcanic areas.

Within the area covered by appreciably thick pyroclastic deposits in the Parícutin region, it is possible to recognize six major environmental facies. These are here recognized as different environments of deposition, each environment depending upon several variable factors, mentioned below, and each environment producing a distinct lithologic facies, usually accompanied by a distinct floral facies. These six major facies are here called (1) the cone facies, (2) the upland facies, (3) the valley slope facies, (4) the valley bottom facies, (5) the lacustrine facies, and (6) the lava facies. Each of these facies will be separately discussed.

The Cone Facies. This facies is restricted to the cinder cone itself and the area surrounding the cone to a distance of about a hundred yards. Lithologically, its materials show greater heterogeneity than any other facies. Composed essentially of subaerially-deposited coarse debris it ranges from large to small bombs and blocks, lapilli, cinders, and very coarse to fine ash. Irregular tongues of lava should be found (though not yet directly observed) interbedded with the coarse volcanic rubble, cinders, and ash near the base of the deposits. Bedding is inclined from nearly horizontal at the base of the cone to as much as 30 degrees or more at the top, dipping radially inward and outward from the rim of the crater. Sudden changes in dips should be expected as the result of surface landslides, mudflows, and extensive slumps, the latter caused by movements of lava tongues from the vent along the base of the cone to its outer margin and beyond. The central portion of the cone facies should be marked by agglomerates representing the volcanic neck or necks. Beyond the periphery of the cone, inclined beds grade into horizontal beds of coarse to fine ash containing scattered bombs and blocks of variable size. Interbedded lava flows are numerous. Judging from the Parícutin cone and from extinct cones in the nearby region, the vertical thickness of the cone facies would range up to 1400 or 1500 feet. Laterally, the facies would extend in all directions as much as 2000 feet or more from the vent agglomerates of the volcanic neck.

The floral facies associated with the cone facies is probably of limited extent, for, though not actually observed, it seems likely that plant remains can be expected only in the very basal beds of the cone. Proximity to the site of violent explosions and to constant showers of coarse volcanic debris from the vent may, in fact, have so completely destroyed the vegetation within this area that recognizable plant remains might be very rare.

The Upland Facies. Throughout the ash-covered area around Parícutin Volcano, the flat, upland divides are the loci of deposition of ash deposits which are characterized by features depending on deposition directly from the air without subsequent disturbance or redistribution by running water. This is here called subaerial deposition. This upland facies is a nearly horizontal to horizontal mantle, diminishing rather regularly in total thickness from the cone radially outward in all directions, from over 12 feet on uplands nearest the cone to less than 1 foot at a distance of 4 miles or less from the cone. Bedding is essentially horizontal, with individual beds ranging from tenths of an inch to several feet in thickness depending upon distance from the vent. Textural range is relatively low, with average textures decreasing from very coarse at sites near the vent to fine or very fine at distances more than 3 miles from the vent. Bedding planes are very regular, except for occasional ripple-marked surfaces representing dry-season wind action.

In this upland facies, the ash deposits not only contain more abundant and better

preserved plant remains than the ash of any other facies, but also preserve the truest record of the forest living in the Parícutin region. This is due to the direct and gentle fall of plant materials from parent trees and shrubs, the subsequent rapid burial of plant materials, and absence of damaging transportation of plant materials by running water. Even under such favorable circumstances, however, the more fragile leaves, such as those of the hawthorn, are destroyed during the process of burial. It was observed, however, that the hawthorn leaves were already crisp and fragile before they were dropped from the parent shrub, so that even a minimum of wind action destroyed them beyond recognition before burial. Other fragile plants remains, including delicate pine catkins and pine needles, were found in a state of perfect preservation.

The Valley Slope Facies. On valley slopes previously cultivated below the forested uplands, ash deposition is a composite of direct subaerial deposition and deposition by running water. In the latter category are sheet deposits, mudflows, channel fills, and alluvial fans. These are the rainy season deposits. Dry season deposits are entirely subaerial. Bedding in the valley slope facies is inclined as much as 35 degrees or more, depending on the initial slope of the buried terrain. Texturally, the ash deposits tend toward a medium grain size in the water-laid deposits to fine in the subaerial layers. Stratification is irregular, in places lenticular, cross-bedded, and channeled. Thickness of the ash mantle increases where slope breaks occur, on the slopes near valley bottoms as well as near the uplands. Laterally, both thickness and texture vary in proportion to nearness to the cone.

The floral facies in the valley slope ash deposits is generally a poor one. Abundant and well-preserved plant remains occur only within about a hundred feet of the forest edge on the higher slopes or uplands. Most of the plant remains are fragmental and are found in pockets, obviously as a result of considerable water transportation and deposition.

The Valley Bottom Facies. In the narrow valley bottoms, the ash deposits are made up chiefly of water-laid channel-fills, lenses, flood-plain deposits, and alluvial fans, with minor proportions of subaerial sheets and aeolian deposits. Local unconformities are common, as a result of re-excavation of previously deposited ash during the rainy season and later deposition of more ash. Textures generally range from medium to coarse ash with occasional pockets or lenses of volcanic pebbles and water-worn lapilli. In places, lava tongues overlie the linear valley bottom deposits, completely eliminating the environment and preventing further ash deposition within it.

Plant remains are fairly abundant but usually not well preserved in the valley bottom facies. The poor preservation is due chiefly to the destructive effects of running water, which is of course the major agent of deposition. Broken twigs and cones, fragmentary leaves, and occasional pieces of larger branches occur mainly in pockets or lenses, often mingled with pebbles. Occasional anomalous occurrences of well-preserved oak leaves and pine needles represent plants growing along the adjacent stream banks. During the dry season, plant remains from these plants were buried rapidly by subaerial ash, and so were not subjected to damaging action by water.

The Lacustrine Facies. During the rainy season there are a few bodies of standing water in the Parícutin region. Some are crater lakes, others are lakes impounded by lava flows. Only the ash deposits of a single crater lake have thus far been examined in the present study. In this, the ash deposits are horizontally bedded, mainly thinly bedded, and very fine grained. Individual beds are generally of uniform thickness for considerable distances laterally. Lenses, channel-fills, and cross-bedded deposits are conspicuously absent. Radially, the lacustrine deposits grade into valley bottom facies or valley slope facies.

Plant remains in the lacustrine deposits were common and well-preserved only near the margins of the deposit where the edge of the living forest was close to the depositional site. Near the center of the deposit, more than a hundred feet from the forest edge, plant remains were sparsely scattered and in good condition, but represented only those plants whose leaves were tough and resistant to destruction by water transport.

The Lava Facies. The occurrence of many pockets of ash deposits within the actual lava tongues was a somewhat unexpected observation. Throughout the solidified blocky lava, there are numerous openings extending from the top downward between the blocks for distances of ten feet or more. During the dry season (and presumably also during the wet season), large quantities of ash fell from direct showers or were blown from ash beds on top of the lava into the openings within the blocky lava. Most of these pockets contained fine ash, usually thinly bedded, with either horizontal or slightly inclined bedding. This intimate association of sedimentary ash and igneous lava is perhaps one which may occur more often than suspected in ancient lavas and pyroclastics of the Parícutin type.

Although rarely found, there are occasional plant remains buried in the ash pockets within the blocky lava. These occur mainly near the margins of lava tongues where the lava edge has filled a valley up to and close to the edge of the upland forest. In a situation of this kind, plant remains are readily shed in considerable quantity directly into the openings in the lava or onto the upper surface of the lava. The latter are in many instances subsequently blown along with fine ash into the lava openings and buried by later ash deposition.

In conclusion, it may be emphasized that observations in the Parícutin region indicate clearly the high degree of selectivity in the processes involved in the burial and preservation of plant remains in the ash deposits. Some environments are definitely better depositional facies for well-preserved, abundant, and adequately representative floral remains. These are the facies which paleobotanists may advantageously search for in ancient volcanic deposits of the Parícutin type. Yet, in all the environments, there is some degree of selectivity in preservation, indicating that even under the most favorable conditions some of the plants now growing in the region will be either unrepresented or very poorly represented in the potential "fossil" assemblages preserved in the ash.

THE BLASHKE ISLAND ULTRABASIC COMPLEX: WITH NOTES ON RELATED AREAS IN SOUTHEASTERN ALASKA*

By MATT S. WALTON, JR.†

The complex of the Blashke Islands described in detail and two other complexes treated briefly are three of at least eight well-defined ultrabasic intrusive masses in southeastern Alaska. Each intrusive mass contains all or parts of a genetically related sequence of basic to ultrabasic rocks, including dunite, pyroxenite, hornblendite, and gabbro with gradational facies and minor variants. These complexes are emplaced in a variety of terranes, comprising both Paleozoic and Mesozoic rocks, all of which have undergone deformation and igneous intrusion during a late Jurassic or early Cretaceous orogeny that culminated in the emplacement of the Coast Range batholith. The ultrabasic intrusions are correlated with the earliest phases of intrusive activity and antedate the more silicic and alkalic intrusions.

* This paper, illustrated with lantern slides, was presented at the meeting of the Section of Geology and Mineralogy on May 1, 1950. Publication has been delayed pending approval of the U. S. Geological Survey. It is published at this time with the permission of the Director of the Survey.

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The complex of the Blashke Islands consists of a nearly circular core of dunite 6,000 to 9,000 feet in diameter encircled by a ring of olivine pyroxenite from a few hundred to 1,500 feet wide, outside of which is a nearly complete ring of gabbro, locally absent but elsewhere up to 1,400 feet wide.

Most of the dunite core contains only chromite and olivine as primary phases. Near the margin, augite is present as a late primary phase and the rock passes into augite dunite or wehrlite. The olivine pyroxenite ring contains essentially only augite and olivine, but near its outer margin a little anorthite and hornblende are present sporadically as late primary phases. The gabbro is irregular in composition and texture, but, where the gabbro ring is wide, the rock tends to pass from an olivine-augite gabbro near its inner contact with olivine pyroxenite into a hornblende gabbro near the outer margins.

Well-defined dikes up to a foot wide of augite dunite and wehrlite cut both the dunite and the olivine pyroxenite, and thin dikes of pyroxenite cut marginal parts of the dunite core. Near the outer margin of the olivine pyroxenite ring there are small, irregular masses and well-defined dikes of coarse gabbroic pegmatite consisting of coarse anorthite, augite, olivine, hornblende, and accessories.

A group of dikes, here called "beerbachite," ranging from olivine gabbro to hornblendite in composition and characterized by aplitic texture, cut the olivine pyroxenite and gabbro. Inclusions of country rock in the olivine pyroxenite and gabbro correspond closely to the hornblende beerbachite dikes in texture and composition. One unusual dikelike rock, cross-cutting olivine pyroxenite and associated with beerbachite dikes, consists mainly of diopsidic augite and andradite garnet.

Within 2,000 feet of the complex, there are a number of dikes and irregular masses of gabbro similar to that in the complex. The country rocks throughout the islands are also cut by numerous dikes of gabbroic to andesitic composition. These are generally porphyritic and ophitic or diabasic in texture. These dikes are metamorphosed by and antedate the complex. Later dikes of andesite-basalt melaphyre cut the complex and are correlated with Tertiary dikes in the region.

The country rocks invaded by the complex consist of Lower and Middle Ordovician graywacke and conglomerate and a lower Silurian sequence containing much conglomerate, graywacke, slate, some limestone, and much andesitic pyroclastic material. A younger Silurian sequence that is largely limestone is unconformable with the older rocks and outcrops at the north end of the islands more than a mile from the complex. All the Ordovician rocks and the rocks of the lower Silurian sequence underwent low-grade regional metamorphism and some folding before the younger Silurian rocks were laid down. Within 100 feet or more of the complex, all country rocks, irrespective of original composition, have been converted into hornfels with granulitic texture and gabbroic composition. The contact metamorphism diminishes in intensity with distance from the complex and becomes negligible within several hundred to a thousand feet.

The contacts between the several rock units of the complex and between the complex and surrounding rocks are irregular in detail but essentially steep. The complex is a nearly cylindrical plug, but may have passed into a domelike structure a short distance above the present level of exposure. The enclosing rocks have been folded regionally and are part of a major structure called the "Kashevarof anticlinorium." The complex is strongly transgressive, cutting across structures in the enclosing rocks, but also has had a doming or upward-dragging effect on the enclosing rocks. Ordovician rocks crop out around the complex and not elsewhere in the Kashevarof anticlinorium. A system of north- to northwest-trending faults of Tertiary age cuts the complex and enclosing rocks. Ten to 20 feet of postglacial uplift is recorded by raised banks of lignitic mud containing marine shells.

The composition of the minerals compris-

ing the principal phases throughout the complex have been determined by measurement of optical properties and comparison with published correlations of optical properties and chemical composition.

All the mineral components of the complex change systematically in composition from the core of the complex outward. Olivine ranges from $Fo_{90}Fa_{10}$ in dunite to $Fo_{51}Fa_{49}$ in granulitized country rock. Augite, hypersthene, hornblende, and biotite tend to become richer in iron with increasing distance from the core of the complex. Plagioclase changes from Ab_4-An_{96} to an intermediate to sodic plagioclase in outer parts of the gabbro ring and in altered country rock. The change in each isomorphous series is toward the less refractory end member with increasing distance from the core of the complex. A systematic variation in composition of mineral phases related to a systematic, nonstratiform arrangement of genetically related rock units is here termed cryptic zoning.

Twelve rock specimens from the complex of the Blashke Islands and two from outlying dikes were analyzed. The norms and modes are given and the analyses are plotted on variation diagrams and a von Wolff diagram. Magnesia is shown to decrease regularly from the core of the complex outward. Lime is at a maximum in the olivine pyroxenite ring and decreases inward and outward. Iron is nearly constant in tenor through most of the course of variation. Alumina is virtually negligible in the peridotitic rocks and becomes a relatively constant major component in the feldspathic rocks. Alkalies are absent in the peridoties and, although subordinate in the feldspathic rocks, increase outward. Correlative variations are shown for minor components. A major break in chemical relationships exists between the feldspathic and the peridotitic rocks, mainly owing to the presence of alumina as a major component in the feldspars, where it substitutes in part for silica, and its absence in the peridoties.

Hypotheses of origin of concentrically zoned ultrabasic complexes involving separate solid, semisolid, and magmatic intrusions, ring dike mechanisms, deformation of stratiform differentiated bodies, crystal settling and convection, and metasomatism are shown to be inconsistent with observed relationships. An hypothesis is advocated by the author, involving the intrusion of an ultrabasic magma of the composition of wehrlite plus some water and other volatiles at a temperature well above 1,000° C. This magma need not be entirely liquid but may consist of solid forsteritic olivine crystals suspended in one or more fluid phases. Upon intrusion into cooler and more silicic and alkalic rocks, a temperature and concentration gradient is set up between magma and surroundings. Transfer of chemical components is supposed to take place along this gradient. Reasons are advanced for supposing that, at the probable temperatures and pressures prevailing, a gas phase escapes the magma and permeates the surrounding rocks. Toward the close of crystallization, the liquid phase may disappear from the nearly crystallized magma and only a supercritical gas phase remain, permeating intergranular spaces in all the rocks. It is suggested that rates of diffusion are rapid in an aqueous gas above critical temperature and pressure for pure water, and that large-scale transfer of chemical components takes place by diffusion in such a medium. It is also suggested that, if the original magma is saturated with water and other volatiles, there will be an outward migration of fluids toward regions of lower vapor pressure, where the magma is losing heat, and this movement of fluids will cause a bulk migration of dissolved solids. The effect of transfer of chemical components along the temperature and concentration gradient by the above methods, or perhaps by other processes unknown to the author, is to bring the magma body to a quasi-steady state, in which olivine is stable in the hot core of the mass and crystallizes outward with cooling, while augite becomes

the stable phase at the cooler margins of the magma body, where the concentration of lime and silica is higher, and crystallizes inward, replacing early-formed olivine. Heat exchange, emanation of volatiles, and outward migration of lime and magnesia gabbroize the surrounding rocks and locally convert some of it into fluid gabbro magma that resolidifies to form the gabbro ring of the complex. The origin of minor rock facies and relationships are developed in detail based on the above explanation of the major relationships, and the origin of an ultrabasic magma is discussed. Beerbachite dike rocks are attributed to the decomposition and mobilization of inclusions of both sedimentary and volcanic country rock.

All the olivine-bearing rocks have been partly serpentinized. Serpentinization is shown to have been a volume-for-volume replacement in two or more stages that took place entirely subsequent to the emplacement of the complex. The initially formed serpentine is shown to be iron-bearing, and a study of analyses of serpentinized peridotites indicates that the combined iron is in the ferric state, substituting in part for silicon as well as magnesium. Part of the ferrous iron originally present in olivine is released as magnetite. Equilibrium data on iron oxides and water suggest that the iron is oxidized during serpentinization by oxygen from the dissociation of water. Field and petrographic evidence suggest, but do not prove, that the initial stages of serpentinization are not related to the weathered profile or to avenues of later hydrothermal or groundwater attack, but are deuteric, and that a later stage is related to weathering.

The regional distribution of ultrabasic intrusions in Alaska and western Canada is summarized. The intrusions are found to fall into groups related to several periods of diastrophism and plutonic invasion. Areally, the ultrabasic masses are grouped near the roof zones of batholithic masses of dioritic to granitic composition, but the ultrabasic intrusions antedate the batholithic rocks. Juxtaposition in space and time indicates that the ultrabasic intrusions and the batholiths are common products of a petrogenetic cycle, but the ultrabasic rocks are emplaced ahead of and above the batholiths; and there is no evidence that can be observed in the field, aside from mere juxtaposition, for differentiation from a common magma. The ultrabasic intrusions and the batholiths are probably derived from quite different sources during the diastrophic process. They are common products of a process, not a magma.

The petrology and structure of the complex of Kane Peak on Kupreanof Island are briefly described. This complex is shown to be a funnel-shaped body with inward-dipping contacts between rock units, but the sequence of rock units from the core outward is similar to the complex of the Blashke Islands. Some observations of the complex of Mount Burnett on Cleveland Peninsula are summarized, indicating that, although the complex is similar to the complex of the Blashke Islands in the sequence and relationships of rock units, it passes into an elongated dome or a tabular mass toward its western end, where the dunite core is overlain by olivine pyroxenite and gabbro. In the complex of Kane Peak, the olivine pyroxenite zone and other outer zones dip beneath the dunite core. In parts of the complex of Mount Burnett, the outer zones overlie the dunite core. These relationships are best explained by a process of differentiation, such as that advanced for the complex of the Blashke Islands, that is independent to a considerable degree of the attitude and structure of the complex and its contacts with enclosing rocks.

SECTION OF BIOLOGY

VIRUSES AND CELLS—A STUDY IN TISSUE CULTURE APPLICATIONS*†

By F. B. BANG AND G. O. GEY‡

A study of the growth of viruses in tissue culture may best be considered under several different headings. These are indicated in FIGURE 1. Dr. Grey has outlined the factors which affect the contact of virus with cells in tissue culture. We should like to point out that the entrance of virus into a cell in tissue culture may consist of much more than a penetration of the theoretical membrane. It may well involve the inclusion of the virus particles within fluid which has been taken into the cell by pinocytosis[1] or the uptake of the particle by the constantly changing waving frills of some cells: the progress of the virus from the periphery of the cell towards the nucleus in the channels, which can be seen both with phase microscopy and electron microscopy. The virus may then begin to multiply. We have no original thoughts on how this then happens. However, study of continuous lines of cell cultures shows us some of the factors influencing this growth. These have had a limited application in the study of virus growth[2] and in only rare instances has a differential effect of virus on different cells, as studied in tissue culture, been observed.[3]

Recently, we have compared the susceptibility of several chicken cell types to the virus of fowl pox and found that epithelial cells were rapidly destroyed by this virus, but that fibroblasts would support a continuous although minimal production of virus for periods as long as 100 days after the original inoculation.[4]

Currently, our interest is centered on a study of the virus of eastern equine encephalomyelitis on different normal and tumor cell lines. Most of these cells have been continuously grown in the laboratory for some five to ten years. A comparison of the normal cell line with its tumor cell derivative[5] has shown that the normal cells are highly resistant to the virus, even when undiluted chick embryo preparations titering 10^{-6} or 10^{-7} on 10-day chick chorio-allantoic membrane are placed directly on these exposed cells.

It is worth re-emphasis that these cells are grown on glass without a fibrin clot and that the outgrowth of cells should present a maximum exposure to particles circulating in the roller tube fluid. With these resistant cells, there is a low and temporary yield of virus. Thus, it is doubtful whether any significant amount of new virus has been produced in these cultures. These cells survive. In contrast, the tumor cell derivative (T333) is destroyed and constantly yields a large amount of virus, the production of which ceases with the death of the cells. Some human tumor cells (D1 Re) and normal rat muscle cell lines have been found to have an equal degree of extreme susceptibility.

Perhaps the most interesting situation, however, is presented by those cell lines which show an intermediate type of reaction. In FIGURE 2 is shown the amount of virus obtained from a rat cell line which has been called altered normal because it has certain cytological characteristics which differentiate it from the normal cell prototype. However, it has not produced new growths on subinoculation into rats. It will be noted that the inoculation of a dilution of 10^{-3} of infected embryo extract or about 1000 LD_{50} of eastern equine encephalomyelitis was sufficient to initiate infection in these cell lines so that virus could be obtained again from the supernatant fluid at any time thereafter. With this

* This paper, illustrated with lantern slides, was one of three presented under this title at the meeting of the Section on May 14, 1951. It is expected that the remaining papers, by G. O. Gey and by J. F. Enders, respectively, will appear in a subsequent issue of TRANSACTIONS.

† This investigation was supported in part by grants-in-aid from the National Institute of Health, U.S.P.H.S., and The Damon Runyan Research Fund.

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growth of virus, there was also concomitant growth of new cells, which in turn served as a substrate for new virus growth.

It is, then, obvious that new cell growth stays ahead of virus growth, but how? If the cells are allowed to degenerate simply because of failure to replace the medium, we get little effect upon the virus yield. It might be well to point out that there are probably more cells continuously present

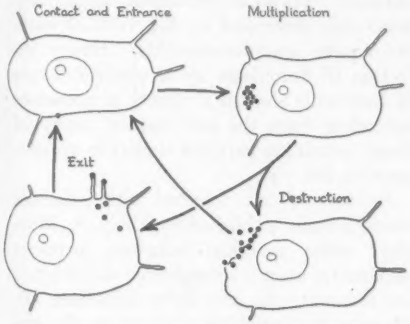

FIGURE 1. A schematic representation of the events involved in the transfer of virus from one cell to another.

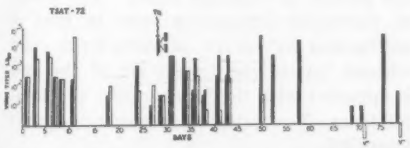

FIGURE 2. The amount of eastern equine encephalomyelitis virus obtained from a continuous roller tube culture of an altered normal rat fibroblast. Cultures were infected on day one. No new cells or virus added to the mixture thereafter. Growth of cells sufficient to warrant splitting culture in two on two occasions. Cells in one tube recovered spontaneously.

than there are virus particles in the supernatant fluid, for a yield of 1000 LD_{50} (10^{-3}) per drop of fluid simply means 10,000 LD_{50}'s (10^{-4}) of virus total. The average moderately well grown roller tube culture may be estimated to contain about 10^6 cells per colony with about four colonies per tube. If infection of the embryo is produced by very few individual particles, then the number of cells would exceed the number of particles by approximately 100 to 1.

Such calculations, of course, are indicative only of the idea that only a few cells in these intermediate cultures are infected and that these yield relatively few virus particles. There has been no evidence in any of the infections of our roller tube strains with this virus that there is any great excess of virus bound up in the tissue culture itself. Some sixteen different cell lines have now been tested for susceptibility to the virus and these tests have confirmed the reproducibility of results with individual cell lines and the variation of susceptibility of different lines. This has no direct relationship to the demonstrated malignancy of the cell line. Some cell lines which have but a moderate infection may spontaneously recover from that infection. When these cells are tested again for susceptibility, they readily become reinfected and yield the same amount of virus that had been demonstrated at the time of the first inoculation (TABLE 1).

TABLE 1
SPONTANEOUS RECOVERY OF INFECTED ROLLER TUBE CULTURES AND REINOCULATION

	1st inoculation					*Reinoculation*	
Dilution	*2 day*	*6 day*	*17 day*	*26 day*	*47 day*	*3 day*	*12 day*
Und.	4.3	1.3	0	0	0	4.8	0.5
	4.3	1.0	0	0	0	4.5	0.5
10^{-8}	3.8	1.0	0	0	0	4.5	0.5
	3.8	1.3	0	0	0	3.5	0.5

Repeated tests of many of these different lines have failed to show that they are susceptible to the virus of Newcastle disease of chickens. The effect of the medium in this case, however, has not been fully analyzed, although the cells were tested in the presence of chick embryo extract.

Cytopathology in Tissue Culture. It is usual in considering the cycle of virus growth to consider that the cell is completely destroyed by the invading and multiplying virus. Study of human fibroblast cells in roller tubes infected with the virus of lymphopathia[6] some years ago showed that cells may maintain large inclusions consisting apparently of innumer-

able individual virus particles and yet survive for a number of days with this load of virus. Similar results were obtained with this virus in some rat tumor cells.[7] Currently, we are attempting to analyze with the use of the electron microscope the changes produced in cells by various viruses.[8,9] From such a continued analysis, we believe certain concepts have emerged. First, with eastern equine encephalomyelitis, it is possible to find some cells with such tremendous numbers of virus particles that it seems that a major portion of the cytoplasm has been converted into virus. Yet often, on the other hand, specific destruction of cells may obtain with but little yield of virus. This latter not only is evident from the low virus titers of the tissue cultures but is also apparent by direct examination of the destroyed cells, which show very few particles identifiable as virus. This destruction of cells with few virus particles is true of both encephalitis and Newcastle disease virus on chick embryo cells. Such a variation in cell lysis may well present a major challenge to the study of virus-cell relationships in tissue culture.

An electron microscope study of the rat fibroblast and tumor cells obtained directly from the roller tubes infected with encephalitis virus and yielding fairly high titers of virus have shown a variety of patterns. Some cells may show rows of apparent virus plastered along the edge of a cell. Others show various stages of disintegration. Particularly interesting is a composite picture of a cell which shows but a portion of the cell completely destroyed and disintegrating with virus lying free. Other particles have been washed over into the intact portion of the cell, and this part of the cell still preserves the detailed fibrous microstructure previously described in this general cell type.[10]

How does a virus get out of a cell into a new one, to continue the process of multiplication? It is obvious that virus may be set free by the disintegration of the entire cell. Are there, however, other mechanisms? As a contribution to the problem of the relationship of filaments to the growth of several viruses, Robinow described the occurrence of "stalked" forms of vaccinia virus in tissue culture.[11] We have seen similar forms in thin sections of chick chorio-allantoic cells infected with the same virus.[12] The spikes which project from a great variety of different cells in tissue culture offer an opportunity for virus exit without complete cell destruction. We have obtained pictures of such spikes apparently selectively destroyed by the virus of eastern equine encephalomyelitis. Heavy infection of fibroblasts by a virulent strain of Newcastle showed a variety of processes extending from the cell surface, many of them containing particles similar in appearance to the virus.

Pictures of cells infected with influenza virus, already published by Dr. J. S. Murphy[9] show spherical influenza particles apparently coming loose from a disintegrating filament. In this latter infection, Dr. Murphy is continuing a study on the significance of these particular filaments in the growth of influenza virus. The point of particular importance here is that a mechanism for escape of virus from cells without immediate destruction of the cell is apparent with the four viruses: vaccinia, influenza, Newcastle, and equine encephalomyelitis.

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SECTION OF MATHEMATICS AND ENGINEERING

ENGINEERING APPLICATIONS OF STATISTICAL EXTREMES*

By E. J. GUMBEL†

The statistical theory of extreme values is able to solve problems of the following type: What is the most probable oldest age at death in a certain population for a given year and a given period? Is this extreme duration of human life a measure of hygienic achievement? Does it increase with time, and how?[5] What is the most probable strongest wind at a given location during a given period of time—a question which is of interest for the building code. What is the most probable largest precipitation for a given region—a question important for agriculture. What is the most probable flood for a given river and a given period?[6] Do the floods increase in time, and how? This question is of obvious interest for the construction of bridges, dams, and hydro-electric plants which have to provide against floods.[2] What is the most probable drought for a given river during a given period of time—a question related to water supply and sewage disposal, and thus to public hygiene.[12] Can quality control be based on the extremes alone? What is the probability that a metal specimen breaks under a given load after a given number of cycles—a question which is fundamental for industrial production.

This enumeration will be shocking to the scientifically minded reader, since it could be interpreted as a claim that this method is a panacea for solving problems arising in actuarial science, climatology, geophysics, public hygiene, and engineering. It is soon realized, however, that the claim of solving a problem common to all these applications by a common statistical method is justified. It is the purpose of these lines to explain the theoretical basis of this method.

In a nutshell, it is a classical procedure applied to relatively new problems arising from small probabilities. The first study in this direction is due to Poisson. In his law of rare events, he gave the distribution valid under the condition that the probabilities are small and the numbers of observations large so that their product remains finite. A similar problem exists within the statistical theory of extreme values.

To develop the theory, consider an example. In a country of, say, five million inhabitants, with a death rate of one per cent, there will be about 50,000 deaths per year. These people die at different ages. One among the 50,000 reaches the oldest age at death, which will be in the neighborhood of 100 years. Assume this age is known for, say, 30 calender years. Then we have 30 largest values differing from year to year. These 30 oldest ages form a distribution of extreme values. Thus, the extreme value taken from a certain initial distribution (the distribution of deaths over the ages) and for a certain number "n" of observations (say 50,000) is a new statistical variable having a distribution of its own. The question is, what is the nature of this distribution?

It is immediately realized that the questions stated at the beginning are all of this logical type and can therefore be solved by the same logical procedure to start from an initial distribution and a given sample size n and to end with the distribution of the extreme values.

A second problem is: Does the extreme value (the oldest age) increase with time, *i.e.*, practically speaking with the number of observations taken over the years? To find a solution, we construct the oldest age reached up to a certain period. This variable increases, by definition, with time. The question of how it increases can be solved only if we know the distribution of the extremes.

Mathematics yield only what you put

* This paper, illustrated with lantern slides, was presented at the meeting of the Section May 18, 1951.

† New York, N. Y.

into them, and nothing more. Therefore, we have to recognize the purely statistical nature of our problem. This means that we are working within the domain of the calculus of probabilities, a closed system where you can derive probabilities from given probabilities, but which does not lead to the solution "security" which the "practical" people have in mind. To amplify this contrast, I may mention a conversation I once had with the chief of an important government agency. In a disgusted way he told me: "We build on unsound ground, and you, Gumbel, want to improve this by introducing gambling into the engineering profession." My answer was: "There is just one small difference between us: you gamble and don't know it. I propose to gamble knowing the risks." Instead of trying to overreach our competence, we have to confine our aim to finding statistical solutions.

The knowledge that we deal with a statistical variable implies the assumption that the fundamental conditions of our observations did not change within the time observed, and will not do so within the time for which a forecast is wanted. Therefore, our theory is a static one. It assumes the continuation of the present situation. If this assumption does not hold, the theory can be modified accordingly. For instance, a known trend could be eliminated.

The exact statistical theory of extreme values is so simple that it can be condensed in a few lines. Let x be a statistical variate, and let $F(x)$ be the probability of a value equal to or less than x. This analytic expression is supposed to be known. Assume that n independent observations are made. Then the product $F^n(x)$ is the probability that all observations are equal to or less than x. One can also say that this value x is the largest among n independent observations, and that

$$\Phi_n(x) = F^n(x)$$

is the probability of the new statistical variate "largest among n observations." As long as n is small and $F(x)$ is known, the probability $\Phi_n(x)$ can be calculated, and the problem is solved.

The asymptotic theory asks: What happens if x is large and n increases without limit? In this case, the probability function of the largest value can be written $\exp [n \log F(x)]$, and the expression in brackets is indeterminate, since one factor, n, goes to infinity, and the other factor, $\log F(x)$, converges to zero. To find the solution for this problem, we have to make certain assumptions regarding the behavior of the initial distribution if the variate tends to very large values. The initial variate is either limited or unlimited. The first assumption can be made if purely physical, not statistical, reasons indicate the existence of such limits. In contrast to the usual opinion, this is rarely the case, since many considerations believed to be physical are, in reality, again of a statistical nature. Some people believe that there is an upper ceiling to the flood which can be obtained by multiplying the strongest precipitation with its longest duration, taking into account the strongest wind and other climatological variates. In reality, if a strongest intensity of rainfall, say x inches of precipitation per hour, has been observed, there is no reason why x plus a small amount could not happen, and the same argument holds for similar variates. If a flood of one million cubic feet per second has happened, there is every reason to believe that a flood of one million and one thousand c.f.s. will happen at a certain later day, which may be far off. The assumption of an upper limit introduces a discontinuity into the variate which is logically untenable.

It is logically and analytically simpler to deal with unlimited variates. From a practical standpoint, it does not matter whether the probability of a certain value or a larger one is 1×10^{-10}, or zero. For all theoretical considerations, however, the assumption of an unlimited variate is simpler, and this assumption is actually made in all utilizations of the normal distribution, Maxwell's distribution, and in

the actuarial functions used in the insurance business. Incidentally, many practical people object to the notion of an unlimited variate and, at the same time, use distributions where the variate has no upper limit.

An asymptotic theory of extreme values can be constructed for limited as well as unlimited variates. The second case is more important. Therefore, it is the only one considered here. In this case, the probability $\Phi_n(x)$ of the largest values converges under certain analytic conditions, with increasing sample size n, toward

$$\Phi(x) = \exp[-e^{-y}].$$

In this formula, y stands for a reduced largest value given by

$$y = \alpha(x-u).$$

where u is the most probable largest value, and $1/\alpha$ a measure of dispersion. Both parameters can be calculated from the observed extreme values alone. Experience has shown that this function fits very well to the observed floods,[1,11] droughts,[12] extreme winds, extreme precipitations,[8] oldest ages,[5] strongest earthquakes, gust loads,[10] *etc.*

Thus, the first problem mentioned before is settled. For the second problem, the forecast, we have to introduce a new statistical function called "Return Period" or recurrence interval. Consider first the classical unbiased die. The probability to obtain a certain point is 1/6. We can also say: If a very long series of observations is made, the chosen point will return, in the mean, after six observations. This value is obtained as the reciprocal of the probability. Consider now the distribution of a continuous variate. Then the probability of a value larger than x is $1 - F(x)$. Its reciprocal,

$$T(x) = 1/(1 - F(x)),$$

is defined as return period, *i.e.*, the mean number of observations necessary in order to obtain, in the long run, once a value greater than x. The return period increases with the variate: for yearly observations, the return periods are years.

Let us apply this notion to the distribution of extreme values. Then the return period of an extreme value equal to or larger than x is

$$T(x) = 1/(1 - \Phi(x)).$$

To utilize this function, a probability paper is constructed showing the extreme variate, for instance, the flood, as a linear function of the reduced variate y and, for large extremes, as a linear function of the return period.[7] Then, the largest value can be extrapolated on a straight line as a function of the numbers of extremes and, since we deal with annual observations, as a linear function of the number of years. This procedure is pseudodynamic. The time enters only in so far as it is related to the number of observations. Consequently, instead of looking forward, we may as well forecast looking backward.

These methods permit the forecast of the most probable flood to be reached within a period of years, and lead to the knowledge of the return period for a chosen design flood appropriate for constructions. Thus, the most important technical problems of flood control can be solved by standardized methods, avoiding the arbitrary procedures previously used. The return periods may serve for actuarial calculations, balancing the cost of a structure against the risk involved.

Since the droughts may be considered as the annual smallest values of the discharges of a river, they can be studied in a similar way to the floods. A sound statistical method for the analysis of droughts is a much-needed tool in the pressing public health problem of stream pollution. The theory determines the probabilities of low runoff and permits a forecast of the most probable severest drought within a given period.[12]

Several recent government publications[1,3,8,9] prove the efficiency of the ex-

treme value theory. The rivers act as if they knew the theory (see the graph taken from Water Supply Paper 1080[11]). Not all engineers, however, act as the rivers tell them to do. Sometimes, it seems as if vested interests were attached to antiquated formulas.

A new and promising field of application of the theory of extreme values is opened by the numerous problems arising from breaking strength of materials.[4] In the classical theory, the ultimate strength is a fixed value, characteristic of the material, which may be calculated from purely physical considerations. This means that the probability of fracture is believed to be zero as long as the stress is smaller than a certain amount, and is unity as soon as this value is exceeded.

All practical experience refutes this opinion. In the fracturing of metals, textiles, and other materials under applied force, the strength of a specimen turns out to be considerably smaller than its theoretical value. It was generally believed that the discrepancy could be eliminated by computing an arithmetic mean taken under constant conditions from different specimens which was then considered as the correct value.

This theory is opposed by the statistical standpoint according to which the breaking strength is a statistical function varying from specimen to specimen under constant conditions for given materials and subject to a specific size effect. In the testing of yarns, the results are influenced by the length of the yarn. In crystalline specimens, the probability of rupturing a material of given volume under a given force depends upon the volume, and the breakdown of capacitors depends upon their size.

This difference between the calculated strength of material and the actual observed values and the size effect resides in the existence of flaws in the body that will weaken it. Since the flaws are distributed at random, with a certain average density per unit volume, a different amount of force will be needed to fracture it at one or another point. Then, the strength of a given specimen is determined by its weakest point. No chain is stronger than its weakest link. This experimental fact is the basis for the application of statistical theories, especially the symptotic theory of extreme values.

In these applications, it is assumed that the number of flaws is large, and that their distribution in the body is independent of their size. Under these conditions, the distribution of the largest flaws is the distribution of the largest values. It is finally assumed that the breaking strength is a

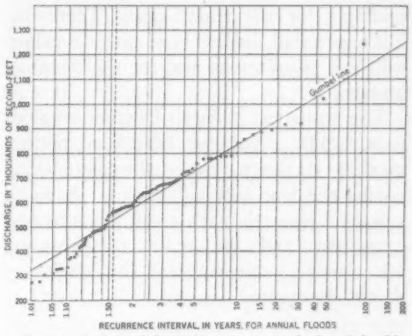

FIGURE 1. Annual-flood frequency graph for Columbia River near The Dalles, Oregon.

linear function of the size of the largest flaw alone, whatever its size may be. The larger the flaw, the smaller the strength necessary for rupture.

Thus, the breaking strength becomes a statistical variate, and its distribution is one of the asymptotic distributions of the smallest values. The problem of establishing the dependency of the strength on the length or volume is equivalent to studying the distribution of the smallest value for increasing sample sizes. The observed frequency can be traced on the probability paper described before, and the most probable smallest breaking strength, asymptotically a linear function of the logarithm of the size and area, can be extrapolated. The parameters in this distribution can be

estimated from the observed breaking strength alone, and are therefore independent of the initial probability law of the flaws.

The dynamic case where a load is applied during a certain number of cycles is more complicated than the static case considered up to now. A new dimension enters. Among others, two new problems arise: How is the load related to the number of cycles for given probability of breakage? Is there a load, called endurance limit, which is so small that the solid can withstand it during an infinity of cycles? Recent experiments have led to satisfactory solutions for the static case. Therefore it is hoped that the theory of extreme values will lead to solutions also for the dynamic problem.

The statistical theory of extreme values is a rapidly expanding branch of mathematical statistics. Soon the astonishing results obtained will look trivial. As in all scientific research, each result leads to new questions, and the main compensation for scientific achievement is the cognizance of our ignorance.

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SECTION OF PSYCHOLOGY

A REFORMULATION OF A BASIC ORIENTATION FOR SOCIAL PSYCHOLOGICAL THEORY*

By DAVID KRECH†

The maturity of a science is perhaps best indicated by the degree to which it lacks autonomy and self-sufficiency. As a science accumulates significant data and as its theory becomes more useful, the boundaries between it and other sciences become more shadowy, and it gradually loses its status as a separate discipline. Its problems, techniques, and theories are taken over by other sciences, and it, in turn, borrows shamelessly (and frequently unconsciously) from the other sciences. At first, this seems to take the form of jurisdictional disputes with the other sciences as to who works what side of nature's street. Later, these disputes are forgotten and, for example, the physicist does not bother to ask for the credentials of the man working next to him on a common problem—credentials which might indicate that his coworker is a chemist or biologist rather than an officially certified and approved "physicist." This, of course, is—*or may be*—scientific progress. I say "may be" because, occasionally, such unification may lead science into blind alleys and false feelings of achievement.

Social psychology today is participating in *two* unification processes. One of these, I believe, has but limited value, while the other promises a great deal more. The first of these—the one about which I have some misgivings—is the attempt to forge a unity, here and now, among social psychology, sociology, and cultural anthropology. The other unification process—the one with most promise—is the attempt to unify social psychology and, *mirabile dictu*, the rest of psychology!

The trend toward unification of social psychology, sociology, and anthropology has been most pronounced in the last two decades. This trend did not "just happen." There are at least three major determinants behind this development.

While the very growth of the separate social sciences predisposed them toward some sort of collaboration, formal organizations and institutes, with prestige and money, were established to hasten such unification. The slow and inevitable, in other words, was "promoted." Thus, in 1923, the Social Science Research Council was organized to encourage interdisciplinary research in the social sciences. In addition, a number of influential research foundations have also promoted interdisciplinary work through grants of research money. The open sesame to the money vaults of these foundations seems to be the magic word "integration," and any request for a research grant which does not include this magic formula risks getting short shrift. Finally, various universities have founded "Human Relations" or "Social Relations" institutes and departments, institutes and departments which are supposed to cut across various social sciences and "cross-fertilize" each other.

The war needs which expressed themselves in the problems of personnel selection, training, propaganda, and attitude measurement also acted as powerful stimuli for unifying the activities of the social sciences. Under the pressure of the great need, many members of the different social sciences who claimed conflicting jurisdiction over these problems were willing, temporarily, to forego their cherished differences and work cooperatively with other social scientists. For most of those engaged in these "war projects," the experience proved to be fairly rewarding, and most of these social scientists came away from this enforced collaboration with much less valley-minded views of their own science than those with which they had entered.

* This paper was presented at the meeting of the Section on May 21, 1951.

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The formation of formal "pressure groups" and the demands of the war, however, acted only as catalyzing agents of a slow and inevitable unification process. The "inevitability" stems, of course, from the compelling fact that all of the social sciences deal with the same basic subject-matter: *the behavior of man in society.* It is highly probable, therefore, that our paths would have crossed at some time or other, even without external push.

All of the above is true and important, but I don't feel that all of this is an unmixed blessing. However, I say this with a great deal of hesitation and reluctance. I hesitate to cast doubts on the desirability of this unification for several reasons. In the first place, many of my colleagues approve highly of this trend and, respecting their judgment, I feel a bit diffident in suggesting that they are uncritically enthusiastic. In the second place, some of these "interdisciplinary" studies have produced useful and perhaps even significant data. Especially is this true in the very important field of current social issues. Finally, in casting some hesitant doubts here, I feel a little bit as if I were out to alienate Santa Claus. After all, most of the money for social science research has come from foundations interested in interdisciplinary studies. And no one likes to discourage Santa Claus!

There remains a fundamental reason, however, why I have my doubts. My ideology of science tells me that lasting progress in science comes only with genuine progress in basic theory. I do not believe that the amassing and classification of facts and the induction and application of general laws is *the* method or foundation or end of science. I believe that the scientist is interested not only in a concise statement of *how* things behave, but even more in *why* they behave as they do. The scientist, for me, is a "tamed metaphysician"—to borrow a phrase from Einstein. Also, it is my considered opinion that, as things stand at present, neither social psychology, sociology, nor anthropology has much in the way of a fundamental theory to answer the question of "why." And it is also my considered and stubborn opinion that we will never arrive at a basic theory of human behavior until we understand the behavior of *individual organisms*, the loci or carriers and creators of all social, psychic, and cultural events. This objective—a basic theory of individual behavior—is *the* task of psychology.

Once we have such a theory, then the unification of these different disciplines will proceed rapidly. In addition, with such a theory, the problem of unification will disappear; for, with a useful theory of behavior, the present disciplines of sociology and anthropology will be seen as but applications of this basic theory to special areas of human behavior. I am *not* asserting that these other social sciences are merely "applied science" and that social psychology is "pure science." I agree with Pasteur, who insisted that there is no such thing as "pure" science and "applied" science but rather there is only science and the application of science. When we get a basic theory of behavior, social psychology *as such* will disappear and what we will have is a science of behavior and various scientists who will seek to apply *this* science to various special problems. If Pasteur was right, and if I am right in my judgment that we have not yet arrived at an adequate theory of behavior, then I am tempted to paraphrase Angyal and to define social psychology, sociology, and anthropology, as of today, as "the misapplication of a nonexistent science."

How are we to achieve this unifying basic theory of behavior? For the answer to this question, I look to the second of the two unification movements in social psychology, the significant attribute of which is that it does attempt to build a theory about individual behavior, but of the *social* individual.

Those familiar with the experimental literature in social psychology are fully aware of the laboratory trend of many current social psychological studies. I

refer to the sort of work which is being done by Bruner and his colleagues at Harvard, to the work which characterized Sherif's early experiments at Columbia, Murphy's work at City College, Klein's work at the Menninger Clinic, *etc.* These social psychologists, and many others, seem to be of the opinion that if we are ever really to understand the social behavior of man, we must have a clear understanding of the basic processes involved in behavior: the processes of perception, learning, memory, motivation, creative thinking, and emotion.

There is nothing new in the psychologist's concern with these processes. The traditional experimental psychologist has long concerned himself with these problems. Nor is there anything new in the fact that the social psychologist is paying attention to these processes. Almost every social psychology textbook, for example, devotes a great deal of space to the laws of perception, motivation, and learning as adumbrated by the experimental psychologist. What *is* new, is that those social psychologists are not "borrowing" those laws from the experimental a-social psychologist, but are defining these problems as basic *social* psychological problems, are themselves doing experimental work with these problems, and are attempting to write a social psychology in terms of *socialized* laws of perception, motivation, and learning.

The major theme of the experimental work of these social psychologists can be stated very simply. Every psychological process of man—his perception, learning, and feeling—must be understood within the social context of the perceiver, the learner, the wanting man. This unification process, of course, is meeting some resistance from the experimental psychologist, who asserts that perception, learning, and memory are within his bailiwick and cautions the social psychologist to stick to his own side of the street.

This position presents a serious challenge to the experimental psychologist. For the experimental psychologist has tended to assume that in studying perception, for example, he was studying a process independent of the social background, affiliations, and values of the perceiver. In so far as he was aware of the influence of these factors, they were seen as interfering, uncontrolled influences—"imperfections" in his subjects. If the position outlined above is correct, however, then we cannot explain away these "imperfections" as nuisances which "mess up" an otherwise good experiment or analysis. The mores of men are just as significant a set of variables as any others, and, therefore, many of the general psychologist's laws and theories must necessarily be in some error, for he has omitted a whole family of important variables in his theoretical formulation.

This position, of course, offers a program for the unification of the work of the social psychologist and the experimental psychologist. For this position says that the social psychologist must concern himself with the data and theory of perception, motivation, and learning—the very same *problems* which have concerned the more traditional experimental psychologist. It also says, however, that the experimental psychologist, *as* an experimental psychologist, must concern himself with the aspirations, the prejudices, and the mores of his subjects, *i.e.*, with the content matter of social psychology —the so-called "social" or "cultural variables."

It is obvious, nevertheless, that this unification, if it is to lead to unified theory, offers tremendous conceptual difficulties. Before this unification took place, it seemed appropriate, for example, to define our laboratory subjects as being relatively intact human organisms of such and such an age. Similarly, it was possible to define our stimuli in terms of their physical dimensions—pitch, decibels, brightness, or chroma. Now, however, that is not enough. Our subjects and our so-called stimuli must be defined in terms of physiology, physics, *and* "culture." And how can we put these three attributes on the same continuum? Yet we must, since this

unification position asserts again and again that every behavioral law must include among its variables something about the "culture" of the organism. To dichotomize these variables and classify some of them as "physiological" or "physical" and others as "cultural" or "social" is an unhappy makeshift that avoids the problem and can lead but to confusion—certainly *not* to theoretical clarity and unity.

This difficulty has been recognized by some psychologists, and various suggestions have been proposed to deal with it. I wish, here, to outline my own suggestion. Before doing so, however, I must issue a major *caveat*. I fully recognize that my proposed solution is only a partial solution. In a sense, it avoids the original problem of describing the stimulus, by shifting the ground to describing the intervening variable affected by the stimulus. Therefore, this suggestion can have interest only for those psychologists who like to play with hypothetical constructs in their model building.

My suggestion simply stated is this: Whatever the nature of the stimuli impinging upon an organism—whether described as "cultural" or "physical"—we are interested in these stimuli only in so far as they affect behavior. Now, when does any stimulus, however described, affect behavior? I am going to assume (as many others have assumed before me) that a stimulus will affect behavior only when it results in some modification of the nervous structure or nervous functioning of the organism. In other words, I am asserting that the least common denominator of all stimulating conditions is found in their effects: in some sort of a neural trace or neural process. And this neural event is a *common* denominator. The neural change resulting from a complicated "cultural" stimulus is of the same essence and varies along the same parameters, as does the neural change resulting from a simple physical stimulus. I propose, therefore, that the most promising orientation for a basic formulation of behavioral theory is neural-physiological. Thus, we come to the seemingly paradoxical conclusion that, when the experimental psychologist attempts to encompass "social" factors in his theory, he seems forced to resort to neurology! For, when we define all of our intervening variables and hypothetical constructs (hypothetical constructs which are correlated with a variety of *different* classes of stimuli) in terms of neural processes, only then will we be able to formulate a unified theory of behavior with variables along the *same* dimensions.

If there is any substantial validity to my thesis, it seems to me that the social psychologist who seeks to unify his science with sociology and anthropology should address himself to the creation of a neurological theory of behavior. As a *social scientist*, seeking sound unification of the social sciences, I would have more faith in a "Department of Neural-Behavioral Relations," combining the data and theories of neurology and psychology than in a "Department of Social Relations," combining the data and theories of sociology, anthropology, and social psychology.

I began this discussion with several words of approval of unification in science. I uttered those words in good faith. I approve of unification, and I think in unification lies our goal and our salvation. We must, however, have a long, long time perspective. Unification, even the right kind of unification, does not necessarily pay off immediately. Should any misguided university social scientist, after reading these words, scrap his "Department of Human Relations" and substitute for it a "Department of Neural-Behavioral Relations," we should not expect any immediate results which will lead to the withering away of sociology, anthropology, and social psychology as relatively independent disciplines. At the present stage of our knowledge, the best we can do in dealing with the problems of social significance which confront us daily is to use what we have—albeit what we have are nothing but refined technological devices based on common sense and ingenious statistical reason rather than

on basic theory. For the time being that is all we have, but it can be useful—tremendously useful.

For those of us who are concerned with today's social issues (and most of us should be so concerned), most of our time should be devoted to wrestling with present problems by the use of present techniques. Some of us, however, at least *some* of the time, should be concerned with *basic* theory and *ultimate* unification. Here I am afraid that the separation between present advances in theoretical understanding of human behavior and its application may be very, very wide indeed. So wide is the separation, that I cannot refrain from recalling the story of David Hilbert as related in Gamov's delightful book, "One, Two, Three . . . Infinity." It seems that David Hilbert, the famous German mathematician, was asked to deliver an opening speech at a "Joint Congress of Pure and Applied Mathematics." This speech was designed to help break down the hostility that, it was felt, existed between the two groups of mathematics. Hilbert began his speech in the following way:

> "We are often told that pure and applied mathematics are hostile to each other. This is not true. Pure and applied mathematics are not hostile to each other. Pure and applied mathematics have never been hostile to each other. Pure and applied mathematics will never be hostile to each other. Pure and applied mathematics cannot be hostile to each other because, in fact, there is absolutely nothing in common between them."

*DIVISION OF MYCOLOGY**

PLANT CHEMOTHERAPY†

By JAMES G. HORSFALL‡ AND ALBERT E. DIMOND‡

Disease may be defined as a "harmful deviation from normal functioning of physiological processes."[1] The agent which incites this deviation is a pathogen. If the pathogen is living, as for example in the case of fungi and bacteria, it may produce disease at any time after it enters the host. For this reason, disease is considered as commencing after the host is entered by the pathogen, whether symptoms of disease have yet appeared or not. Chemotherapy is defined as treatment of the host with a compound so that the action occurs inside the host when the host is diseased. This action may (a) kill the pathogen as it enters the host, (b) rid the host of an established pathogen, or (c) mitigate the symptoms of disease.

Chemotherapeutic action in the plant may be systemic or topical. If action is at the site of application, action is topical. If action occurs at a point other than the site of application, action is systemic. The diseases caused by parasites may themselves be localized or they may be systemic in the plant. Whether a chemotherapeutant is used which has systemic or localized action in the host is related to whether the disease being combated is of the systemic or localized category.

Examples of topical chemotherapy against local diseases are the cure by Bolley of potato scab by soaking the diseased tuber in mercuric chloride solution,[3] and the cure of rust gall on cedar with sodium dinitro-o-cresylate by Strong and Cation.[36] The cure of crown gall tumors on tomatoes by the same compound,[2] by penicillin,[4] streptomycin,[18] and by folic acid analogues which are effective on animal cancers[8] are also illustrative.

The systemic chemotherapy of localized diseases has been observed with a variety of compounds and diseases. Thus borax, when fed to wheat plants, reduced leaf rust and strip rust,[17] and sodium thiosulfate reduced the severity of powdery mildew when injected into apple.[28]

A number of organic compounds have been used as therapeutants, among which may be mentioned Gassner and Hassebrauk's[16] report of the reduction of wheat rust with sodium toluene sulfonamide and Sempio's[29] ability to increase the resistance of beans to rust by means of caffeine and theobromine. Phenoxyacetic acids have been reported as chemotherapeutants by a number of workers. Crowdy and Wain[7] noted that 2,4,6-trichlorophenoxy acetic acid reduced the rate at which lesions produced by *Botrytis fabae* spread on leaves of the horse bean, and Stewart[31] observed a decrease in the incidence of *Alternaria citri* on citrus fruits in storage after the trees which produced them were sprayed with 2,4-dichlorophenoxy acetic acid or 2,4,5-trichlorophenoxyacetic acid.

A group of 1(substituted)-3,5-dimethyl-4-nitrosopyrazoles, when suspended in water, were absorbed by one leaf of a tomato plant and translocated to another, and the latter leaves were less susceptible to *Alternaria solani* as a result, according to McNew and Sundholm.[26]

Systemic insecticides have been developed which are similarly absorbed by the plant and kill insects which feed upon foliage by sucking. In this manner bis (bis dimethyl amino phosphonous) anhydride may be rendered immune to attacks by sucking insects for a considerable time (Ripper,[27] Martin[25]).

Systemic diseases have been attacked by systemic chemotherapeutants. Thus, Howard[22] has used diaminoazobenzene dihydrochloride injected into maples to combat the bleeding canker disease of maples. Feldman, Caroselli, and Howard have proposed a mixture of calcium hydroxide, urea, a salicylate, and an azo dye as a chemo-

* There was no meeting of the Section of Oceanography and Meteorology during the month of May.

† This paper, illustrated with lantern slides, was presented at the meeting of the Division on May 25, 1951.

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therapeutant for Dutch elm disease.[14] 8-Quinolinol benzoate has been used commercially for the same purpose after the work of Horsfall and Zentmyer[20, 21] and of Dimond *et al.*[11] demonstrated its usefulness. Although none of these represents a final solution to the problem of Dutch elm disease, they are of value while the search for more effective treatments goes on.

For *Fusarium* wilt of tomato and of carnations, both 4 chloro-3,5-dimethyl phenoxy ethanol and 2-norcamphane methanol have been effective under experimental conditions[9, 34] (and n-octadecyl trimethyl ammonium pentachlorophenate has repeatedly reduced the severity of *Fusarium* wilt of tomatoes[6]).

Virus diseases of plants have similarly been controlled experimentally. Stoddard has shown that $CaCl_2$, $ZnSO_4$, and sulfanilamide all inactivate the virus of X disease of peach in diseased buds when they are budded into healthy trees treated with these compounds.[33] Limasset[23] and Locke[24] have both found derivatives of 4-chlorophenoxyacetic acid of value in treating such potato virus diseases as the X and Y virus and leaf roll. Takahashi[37] has reported the effectiveness of malachite green in retarding the multiplication of tobacco mosaic virus in tobacco leaves.

Modes of Chemotherapeutic Action

A chemotherapeutant may be effective through its interactions with the host, with the pathogen, and with toxins which the pathogen produces. These interactions may be dependent or interdependent in specific cases.

Relations of the Chemotherapeutant to the Host. The chemotherapeutant may itself be toxic to the host, and the relation of the tolerated dose to the effective dose must be determined as a first step in working with a new compound. This is readily done by standing cuttings in solutions which are graded in concentration of the compound under test, or by similarly measuring the effect of the compound upon seedlings or mature plants growing in sand or soil.

At dosages less than are obviously phytotoxic, the chemotherapeutant may exert its effect by altering the resistance of the host to disease. One of the ways in which this may be done is illustrated by the effects of 2,4-dichlorophenoxy acetic acid and related compounds upon plants. This compound has been shown to result in an increase in the nitrate content of plant tissues.[32] It is similarly known that *Fusarium* wilt of tomato and other diseases become more or less severe in a plant as its nutritional balance and level are changed.[35, 39] It is, therefore, quite possible that the effect of certain chemotherapeutants may be indirect and operate through the nutrition of the plant.

The chemotherapeutant may be altered by the plant. The systemic insecticide bis-(bis dimethyl amino phosphonous) anhydride appears to be less toxic to insects when applied directly to them than it is when insects feed upon the plant which has absorbed it.[25, 27] There is a similar relation of 4-chloro-3,5-dimethyl phenoxyethanol for *Fusarium* wilt of tomato.[10, 19]

Relation of the Chemotherapeutant to the Pathogen. A number of the chemotherapeutants which have been referred to previously are directly toxic to the pathogen. Lime sulfur, copper sulfate, penicillin, the nitrosopyrazoles, and the systemic insecticides are all known to be toxic to the pathogen to a greater or lesser extent. Direct toxicity to the pathogen is an underlying assumption in the work of Crowdy and Wain.[7] These workers have explored the relation between structure and activity of compounds having hormone activity in plants. By modifying structure to minimize formative effects, but to make maximal the fungitoxicity and translocatability in the plant, they have developed certain aryloxy aliphatic acids as systemic fungicides. Here may be noted 2,4,6-trichlorophenoxy acetic acid which, upon being absorbed by the roots of horse beans, retards the rate of development of leaf spots caused by *Botrytis fabae.*

There is no necessary relation, however,

between toxicity to the pathogen and chemotherapeutic activity. In fact, for *Fusarium* wilt of tomatoes, these properties were compared for a series of forty compounds and no correlation at all was found.[10, 19]

Relation of the Chemotherapeutant to Fungus Toxins. A number of diseases are considered to injure the host because the pathogen secretes toxins. One method of measuring chemotherapeutic activity depends upon measuring the ability of compounds to neutralize these toxins.[15] This method is rapid and simple and chemotherapeutants of promise have been developed through its use.[14] Possible shortcomings of this method are that compounds of promise which have a different mode of action may be missed through its use, and that the investigator must be certain that the phytotoxic fractions which are produced *in vitro* by fungi are also produced in the diseased host.

Assaying Chemotherapeutants in the Host

Alternative to measuring chemotherapeutic activity by the ability of a compound to neutralize toxins produced by the fungus is a method which measures the ability of a compound to reduce the severity of disease by applying it directly to the host and then inoculating the host with the pathogen.[5] Slow as the method is, it is a direct measure of chemotherapeutic activity by any mode of action. Such procedures have been used to estimate chemotherapeutic activity of compounds against crown gall on tomatoes,[13] bacterial blight of beans,[12] and *Fusarium* wilt on tomatoes.[9]

By this method, the compound is applied to the medium in which a plant is growing for a period of days, after which the plant is inoculated with the pathogen. Extensive experiments have shown that distribution of the chemotherapeutant is more uniform and that results of treatment are more pronounced and more uniform when the compound is absorbed through roots than when it is injected into the vascular elements of the stem.[10] For this reason, all compounds are now applied by watering the plant and permitting them to absorb the compound through roots.

Distribution of Chemotherapeutant in the Plant. Some compounds are more freely translocated in plants than others are. The translocatability of 2,4-dichlorophenoxy acetic acid and related compounds is well-known. Compounds with these properties become systemically distributed in the plant. In contrast, other compounds appear to be strongly adsorbed on cell surfaces or within cells and remain quite fixed to these tissues.[6, 19]

There is evidently a relation between the kinds of disease which may be best treated with a chemotherapeutant and its distribution in the plant. For example, the *Fusarium* and *Verticillium* wilts which are caused by fungi borne in the soil and entering the host through its roots may be more readily controlled chemotherapeutically when a compound is introduced into the plant so that its concentration is maximal in the root system and minimal elsewhere. In contrast, a fungus which is inoculated into aerial portions of the plant, as is *Ceratostomella ulmi*, the fungus causing Dutch elm disease, may be best combated with the translocated therapeutant.[6, 10]

Stability of Chemotherapeutants in Biological Systems

In the plant, many chemotherapeutants will be changed. Some are increased in their effectiveness against the pathogen, whereas the effectiveness of others is reduced. The reduction in activity with time in the plant may result from simple dilution of the compound as the plant grows, accumulation in one tissue at the expense of another which requires protection, and from slow breakdown of the compound in the host. All of these causes make necessary repeated applications of the chemotherapeutant at intervals.[9] The persistence of the systemic insecticide bis-(bis dimethyl amino phosphonous) anhydride is unique.[25, 27]

Similarly, some chemotherapeutants are effective when applied to plants growing in sand but are quite inert when applied to plants growing in soil.[10] N-(4-nitrophenyl)-3,4-dichlorobenzene sulfonamide behaves in this manner when applied to tomatoes for control of *Fusarium* wilt and to chrysanthemums for control of *Verticillium* wilt. Other compounds may be quite stable in the soil. No better example of such behavior could be cited than that of benzene hexachloride.[30, 38]

All of these and other factors require studies of the timing requirements for applying a chemotherapeutant. How frequently a compound must be applied depends upon conditions of use. Generally, the compounds now known have short-lived ability to act in the plant. This may minimize food and drug aspects of the chemotherapy of plant diseases, but it increases the handicap of using the method in controlling plant diseases.

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SECTION OF ANTHROPOLOGY

PREHISTORIC CARIBBEAN CULTURE CONTACT AS SEEN FROM VENEZUELA*

By IRVING ROUSE†

During the summer of 1950, J. M. Cruxent, Director of the Museo de Ciencias Naturales in Caracas, and the author undertook joint archeological excavations in Venezuela. Two of our sites—Manicuare, on the Peninsula of Araya off the northeast coast, and Saladero, just above the delta of the Orinoco River—showed similarities with Antillean culture. It is of interest to consider the significance of these two sites for the problem of prehistoric cultural contact between South America and the West Indies.

The site of Manicuare had been discovered by Cruxent during the winter of 1945–46, while he was member of an expedition from the Sociedad de Ciencias Naturales La Salle in Caracas. He collected a number of shell and stone artifacts there without, however, finding any pottery (Gines *et al.* 1946, pp. 157–200). As Walter Dupouy subsequently pointed out (in Willey, 1948, p. 279 and Johnson, 1948, p. 371), these artifacts bear "a close resemblance" to material from Ciboney sites on the Isle of Pines, Cuba, which are also nonceramic. They raise the question whether the inhabitants of Manicuare may have been related to the preceramic Ciboney Indians of the Greater Antilles.

The site is a shell midden, situated just south and west of the present town of Manicuare. While no more than 35 centimeters deep, the refuse extends for over a quarter of a mile along the shore. We dug four trenches, each consisting of four 2-meter square sections divided into two 25-centimeter levels.

Our first objective was to determine whether the site actually lacked pottery or whether Cruxent had overlooked potsherds in 1945–46 because he made only surface collections. Our excavations yielded no pottery at all, although we found large numbers of sherds in two neighboring sites where we also dug. Thus, we are able to confirm that Manicuare is nonceramic, if not also preceramic. So far as is known, it is the first such site to be found in northern South America.

Our second objective was to determine the nature of the culture represented in the site, for the purpose of comparison with the Ciboney cultures of the Greater Antilles. I am unable to present a definitive outline of the culture at this time, since the Manicuare specimens were left in Venezuela to be studied by Cruxent, but what follows is a tentative inventory.

Since the few bones were mostly of fish, it is presumed that the site was a fishing village, like the modern town of Manicuare. The one burial was primary, flexed, and lay in the refuse without grave objects. The principal type of artifact obtained is the shell gouge, consisting of a triangular section of the outer whorl of the conch (*Strombus* sp.), beveled along its lower, curved end (FIGURE 1, a). Dishes, cups, wedges (perhaps used for opening shellfish), hammers, and an incisor-shaped pendant are also made of the conch (FIGURE 1, e; Gines *et al.*, pp. 178–81). In addition, we obtained clam shell scrapers and shell beads in various stages of manufacture (FIGURE 1, c). Bone projectile points were fairly common (FIGURE 1, d), and the stone artifacts included unretouched flakes, a metate and mano, hammers (FIGURE 1, b), and a pitted slab resembling a petroglyph.

In comparing this assemblage with the Ciboney remains in the Greater Antilles, it is essential to discriminate among the seven types of Ciboney culture which are currently recognized. The two closest to Venezuela, Krum Bay in the Virgin Islands

* This paper, illustrated with a Kodachrome movie and slides, was presented at the meeting of the Section on May 28, 1951. It is a contribution from the Caribbean Anthropological Program of Yale University, and acknowledgment is made to the Wenner-Gren Foundation for Anthropological Research for its grants in support of the Program.

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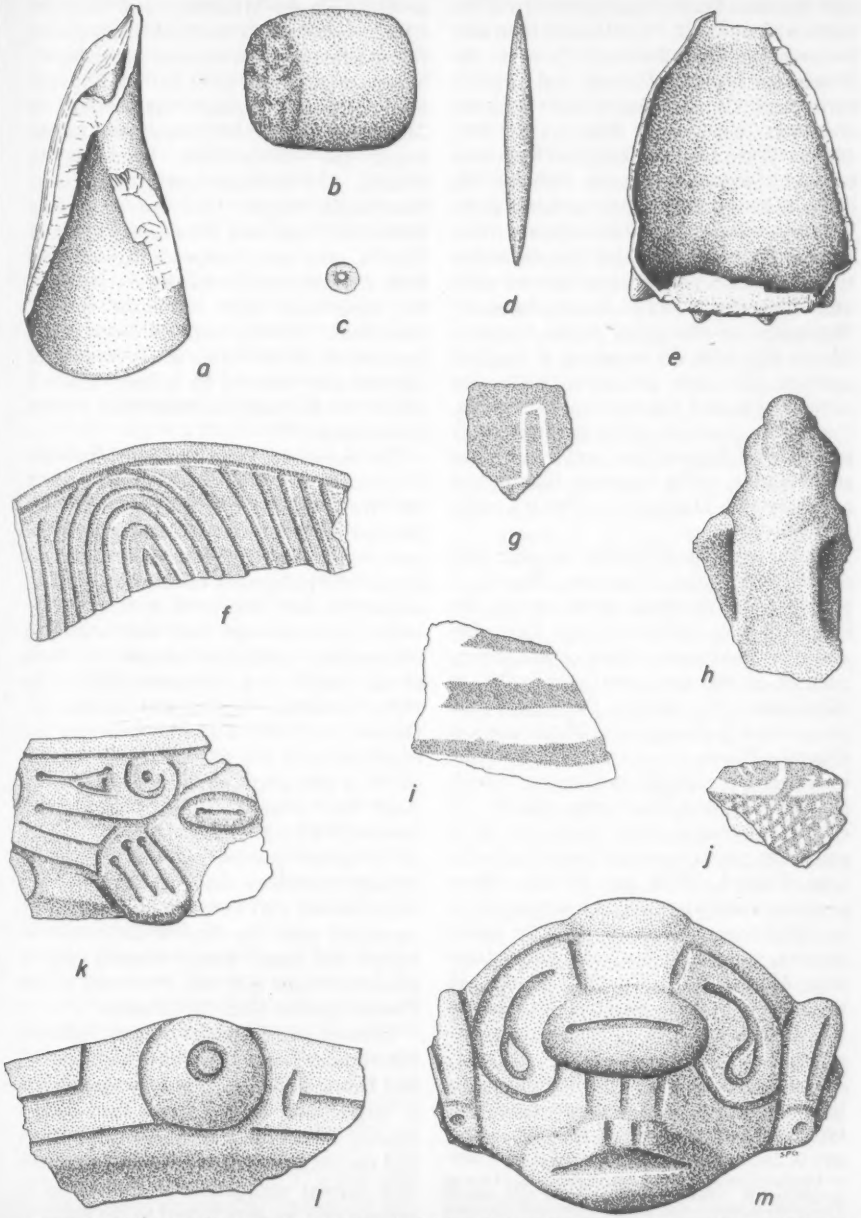

FIGURE 1. Artifacts from Manicuare (a–e) and from the Saladero (f–j) and Barrancas (k–m) occupations at the site of Saladero, eastern Venezuela. (5/9.)

and Coroso in Puerto Rico, are shown at the bottom of FIGURE 2. Continuing from east to west, we find Railroad Cave in the Dominican Republic, Cabaret and Couri in Haiti, and Cayo Redondo and Guayabo Blanco in Cuba (Rouse, 1948, pp. 499–501, 1951). When Dupouy compared Cruxent's original Manicuare collection with the Isle of Pines, he was referring to the last of these Ciboney cultures, Guayabo Blanco. Our finds confirm his impression that there is a strong resemblance between the two cultures. All of the traits listed above for Manicuare are also found in the Guayabo Blanco sites with the exception of the shell pendant, the bone projectile points, the stone metate, and the supposed petroglyph. Conversely, there are only a few traits, such as retouched flakes of flint and possibly the stone mortar, which Guayabo Blanco does not share with Manicuare. This is a striking similarity.

On the other hand, it must be taken into consideration that Guayabo Blanco is further from Venezuela than any of the other Ciboney cultures except Cayo Redondo and that none of these other Ciboney cultures shows any great similarity to Manicuare. For example the type of shell gouge which is distinctive of Manicuare and Guayabo Blanco, is not found in any of the intervening preceramic cultures or, indeed, anywhere on the intervening islands, although Barbados and Carriacou have yielded gouges of a more advanced, celt-like type (Osgood, 1942a, pp. 40–41). Moreover, no traces of preceramic occupation of any kind have yet been found in the Lesser Antilles, comprising over half the distance from Manicuare to Guayabo Blanco, although two of those islands—Trinidad and Martinique—have been intensively investigated.*

The facts of typology and distribution are thus in conflict. On the one hand, the typological resemblances strongly imply one of the following theories of direct connection between Manicuare and Guayabo Blanco: either the Ciboney, having migrated from Florida to Cuba as many archeologists assume (*e.g.*, Rouse, 1949, pp. 125–7), may have continued through the Antilles to Manicuare, or else they may have moved across the islands from Venezuela to Florida. On the other hand, the facts of distribution suggest that the preceramic Indians of Cuba and Manicuare, if not of Florida, may have independently achieved their resemblances by adapting cultures of the same basic type to similar ecologic conditions. Further work will be required, particularly on the islands where preceramic cultures now seem to be lacking, before a choice can be made between these various alternatives.

The second site to be considered from the standpoint of possible relationships with the West Indies is Saladero, which lies just north of the town of Barrancas on the west side of the lower Orinoco River. Potsherds with elaborately modeled and incised decoration were discovered in this vicinity about ten years ago, and they attracted considerable attention because of their artistic quality (*e.g.*, Antolínez, 1940). In 1941, Cornelius Osgood and George D. Howard, of Yale University, made test excavations at the site of Los Barrancos, which is just south of Barrancas, and defined the ceramic style (Osgood and Howard, 1943, pp. 95–111). Osgood (1942b, p. 3) further pointed out that Barrancas pottery resembles that of Erin Bay in Trinidad and that both are probably to be correlated with the Arawak Indians, who moved out from South America during prehistoric time and took over most of the Greater Antilles from the Ciboney.

Cruxent and I chose to dig at Saladero rather than Los Barrancos, where Osgood and Howard had excavated, because it was a larger and deeper site. The deposit extends for over half a mile along the river and reaches a maximum depth of 1.8 meters. The current occupation of the site is so intense that we were forced to dig either in cultivated fields or in people's yards.

* While returning from Venezuela last summer, I had an opportunity to examine the situation on both islands. Twenty-two habitation sites are now known on Trinidad and 24 on Martinique. All have yielded pottery. (Two additional Trinidad localities which have yielded only flint chips may be dwelling sites, but this remains to be proved.)

Altogether, we made nine excavations, which were similar to those at Manicuare, except that they extended to a maximum depth of eight 25-centimeter levels. These yielded the remains of two occupations, an earlier one of a new type for the region, which we have named Saladero, after the site, and a later one, marked by the distinctive modeled-incised pottery of the region, which we will simply call Barrancas (FIGURE 2).

Traces of the Saladero occupation were encountered in the second of our excavations, but it was only in the seventh that we obtained any considerable amount of Saladero refuse. In Excavation 7, beneath a layer of Barrancas refuse 1.5 meters thick, we struck half of a small midden, measuring about 4 meters in diameter and 50 centimeters high, which consisted entirely of Saladero-type material. Our excavation was subsequently expanded to take in the other half of this midden.

The potsherds from the midden are relatively thin, fine, and hard (FIGURE 1, f–j). Many have a sinuous profile, combining a convex body section with a concave, outflaring shoulder (FIGURE 1, f). They lack flanges, *i.e.*, outward extensions of the vessel wall at an angle to the rim. Decoration consists mainly of crude white-on-red (FIGURE 1, g, i) or, more rarely, red-on-white (FIGURE 1, j) painted designs. In addition, there are a few red-painted and simple, curvilinear incised designs (FIGURE 1, f), as well as crudely modeled lugs, most of which are attached to vertical strap handles (FIGURE 1, h).

Some time must have elapsed between the formation of the Saladero midden and the accumulation of the Barrancas refuse, for the latter lay unconformably on the former. Indeed, it is probable that the Barrancas occupation has intruded into the area, for its pottery is quite different from that of the Saladero occupation and we obtained no evidence of a transition between the two either at Saladero or in the 15 other sites of the vicinity represented among our surface collections.

The Barrancas pottery was easy to distinguish from that of the Saladero style. It is relatively thick, soft, and coarse (FIGURE 1, k–m). The vessel sides tend to be single-curved instead of sinuous and are commonly topped with flanges (FIGURE 1, l). Modeling and incision rather than painting are the characteristic techniques of decoration, and the designs are much more complicated (FIGURE 1, k, m). Variations among the excavations, as well as from bottom to top of the refuse, make it possible tentatively to distinguish four stylistic periods, which will be called Barrancas 1–4 (FIGURE 2). These can be defined only in general terms, pending the detailed ceramic analysis. Barrancas 1 has the simplest modeling and incision, as well as the only white-on-red painting. Modeling reaches the peak of its elaboration in Barrancas 2 and incision in Barrancas 3 (cf. FIGURE 1, m and k). Barrancas 4, with which we found possible Spanish trade objects, is degenerate. Osgood and Howard (1943, Figure 22) illustrate modeled heads characteristic of Barrancas 2, which they purchased during their 1941 visit to the region. On the other hand, the material they excavated at the site of Los Barrancos equates with our Barrancas 3.

Artifacts other than pottery were rare, and an adequate sample was obtained only for the Barrancas occupation. They include clay griddles for baking cassava, stone celts and beads, bone projectile points, and shell dishes. It is noteworthy that none of these artifacts is carved or shows any traces of the highly developed ceremonialism of the Greater Antilles. The burials were correspondingly simple. As at Manicuare, they were flexed and without grave objects.

A series of local ceramic sequences in various parts of the Antilles are available for comparison with the Saladero-Barrancas succession (FIGURE 2).* Summarizing these sequences, we may distinguish three

* Figure 2 is based upon a series of previous charts developed in connection with the research of the Yale Caribbean Program: Howard, 1947, Table 2; Rouse, 1947, Figure 1; Rouse, 1948, Table 1; and Rouse, 1951, Figure 2. The Martinique column refers to information supplied by Father Robert Pinchon (personal communication).

main periods in the development of Antillean ceramics, the first characterized by white-on-red painted decoration, the second by variation in decoration from place to place, and the third by an emphasis on modeling and incision. The first and third are considered to be horizons, corresponding to the horizon styles of Peruvian archeology (*e.g.*, Willey, 1945). Since the white-on-red horizon occurs at the beginning of each ceramic sequence, it is thought to mark the migration of the first pottery-making Indians, *i.e.*, the Arawak, from South America into the Antilles. There

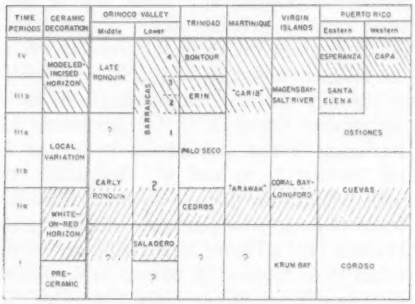

FIGURE 2. Preliminary correlation of archeological sequences in the Orinoco Valley and the West Indies.

followed a period in which the Arawak on the various islands seem to have pursued their own trends of development or were influenced by other groups, as in Haiti, where Ciboney-like designs appear on the pottery (Rouse, 1951). Finally, the modeled-incised decoration, which had been present sporadically during the two preceding periods, became standardized in two distinct forms, one in the Lesser and the other in the Greater Antilles.

As FIGURE 2 indicates, the Saladero occupation on the Orinoco fits into the white-on-red horizon, while the Barrancas occupation extends through the subsequent period of local variation into the modeled-incised horizon. In other words, the ceramic sequence on the lower Orinoco corresponds closely to that in the West Indies and the two may be considered parts of a single ceramic area.

By contrast, only the Saladero pottery appears to be related to material further up the Orinoco. It shares most of its features with the early pottery at the site of Ronquín, excavated for Yale by George D. Howard (1943). Since, however, Early Ronquín also has somewhat more elaborate modeling and incision, we believe that it is more recent than Saladero (FIGURE 2). Late Ronquín pottery, on the other hand, is entirely unlike that of the Barrancas style. Its resemblances are instead with the Arauquín style further up the Orinoco and, in turn, with ceramics on the lower Amazon (Howard, 1947, p. 37; Evans and Meggers, 1950, p. 9).

The main effects of our discovery of the Saladero occupation, then, are to fill a gap in the distribution of white-on-red painted pottery between the middle Orinoco and the West Indies and to provide a simpler form of the pottery, which is more likely than Early Ronquín to have been ancestral to the white-on-red styles of the Antilles. Thus, it strengthens the case for origin of the insular Arawak on the Orinoco since, as we have seen, the white-on-red horizon apparently marks the course of migration of the Arawak into the Antilles.

Our work on the Barrancas occupation confirms Osgood's correlation of the pottery of that occupation with the modeled-incised ceramics of the Antilles. It is interesting to note that the closest similarity is between Barrancas 1 and the late Palo Seco ceramics of Trinidad. These two are so much alike as to suggest a common origin—possibly in Trinidad, since a transition from the earlier white-on-red horizon is present there but not, as already noted, on the lower Orinoco. Such an hypothesis is not inconsistent with the historical references to the effect that the Indians of Barrancas came from Trinidad (Lovén, 1928, pp. 713–14, 724–5). The alternative would be that Barrancas pottery is derived from Early Ronquín, further up the Orinoco. This is less likely, however, because the resemblances between the two are not so close as between Barrancas and Palo Seco.

Once Barrancas pottery was established on the lower Orinoco, it seems to have followed its own course of development, for Barrancas 2–4 are distinct from the contemporaneous styles in Trinidad. Our grouping of both within the modeled-incised horizon seems, therefore, to be a matter of common ancestry and parallel development rather than diffusion from one region to the other.

To summarize, our material from the preceramic site of Manicuare has strong typological resemblances with the Guayabo Blanco type of Ciboney culture in Cuba, but it is difficult to reconcile these resemblances with the absence of similar cultures in the intervening areas. Our excavations at the ceramic site of Saladero confirm the existence on the mainland of the white-on-red and modeled-incised horizons of the Antilles. It is felt that the former, represented by the Saladero occupation of the site, marks the course of Arawak movement into the islands. The modeled-incised, or Barrancas, occupation, on the other hand, appears to have been the result of a reverse movement from Trinidad.

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AMERICAN INDIAN WARFARE*

By MARIAN W. SMITH†

Wars are frequent in world history and appear among many peoples of diverse backgrounds. Yet, partially because of the complexity of the data and partially because of cultural biases and involvements in particular wars, warfare is probably the least adequately studied of all social phenomena. The present paper represents a beginning attempt to rectify this situation, and aims to analyze the features associated with the total phenomenon of war as it occurred among American Indians. Because Plains warfare happens to have been fairly well described, and the ethnological materials are generally richer for North America than for Middle and South America, the Plains and North America are often used as points of departure in the discussion.

Descriptive studies of war data for the New World may be conveniently reviewed under three headings: (a) the game of war, or social contests; (b) mourning-war; and (c) the war party. Considerable telescoping is necessary to handle all of the aspects of war which fall within these large categories and some reorientation of interpretation is inevitable. The materials, however, have a peculiarly New World flavor and, although some of the practices overlap with warfare practices in other parts of the world, taken together they represent unique syndromes not to be found elsewhere. It is striking, for instance, that even detailed study of the data does not lead to recognition of some of the reasons for going to war which are most familiar to us from our knowledge of European history unless we ask the further question: "Why did American Indians fight?" The fourth section of this paper is, therefore, devoted to "causes of war."

During the examination, it is found that mourning-war breaks down into two discernible parts and that one of these occurs in combination with the war party. Feud is also seen to be separate from the "revenge" of mourning-war and to combine with different reasons for going to war. Consequently, four sets of distributions to be considered in handling American Indian warfare are indicated. These differ from the headings under which the materials were first examined and may be briefly presaged here as: (1) "feud" or simple reprisal; (2) the social contest; (3) the sequence of behaviors which is, for want of a better phrase, referred to as "shame-aggression;" and (4) a new "mourning-war," now more carefully, and more inclusively, described.

Any analysis of war today will inevitably be asked to furnish conclusions relevant to a solution for war, and it so happens that the "social contest" described here for the New World does supply information on possible substitutes for war. Comment on the way in which these substitutes seem to have operated, and certain additional comments stemming directly from the American Indian data, but probably applicable to a wider field, are therefore added at the end of the paper.

Social Contests—the Game of War. One of the perennial problems of Plains warfare is the extent to which it may accurately be described as a "game."* All ethnographers of the Plains, with first-hand acquaintance of the people, seem to have used this description at one time or another. "Play" and war have been closely connected by some cultural theorists (see especially Huizinga, 1949†), and it is true that Plains warfare showed a euphoric energy often associated by Europeans and Americans with major sporting events. Yet various criticisms have been directed against the sportive or inconsequential implications of the term as applied to Plains warfare (Newcomb, 1950, p. 329; Smith, 1938, pp. 431–432; *etc.*), and Plains ethnographers seem to have intended more than this in

* This paper was read at the meeting of the Section of Anthropology on January 22, 1951, and, through the courtesy of the author, was presented for publication at this time.

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* Woodland tribes bordering on the Plains shared almost all of the Plains traits of warfare. So far as war practices are concerned, the western Woodlands tribes and the Plains constitute a single distribution. Unfortunately, space does not allow for the documentation of many such points in this paper, and the author must frequently rely upon the previous knowledge and interest of the reader and the few summations of data which are available.

† I am indebted to William S. Evans for this reference.

their use of the word "game." Certainly the warrior used the system of graded war honors (Smith, 1938, pp. 426–434) to establish his status within the group. This systematization was usually both precise and explicit. Military exploits were defined and ranked for each tribe, and enough overlapping occurred so that they also had considerable intertribal recognition. Common war honors were coup counting, killing, scalping, and stealing away with important personal property, such as a buffalo- or war-horse, gun, or medicine shield. The theft of run-of-the-mill horses, as opposed to highly trained mounts which were carefully guarded by their owners, counted as a war honor only among the Cheyenne and Crow. Other factors than military exploits, such as the assistance given the warrior by wealthy relatives or the supernatural aids available to him, were culturally subsumed under the guise of war.

Plains warriors first recounted their exploits at the scalp dance which followed the return of the successful war party. Thereafter, each exploit was added to the warrior's previous repertoire and important men had impressive lists which they recounted frequently upon public occasions. This recounting of war honors was often an elaborate reenactment of the military situations involved and some men developed considerable virtuosity in their performance. Personal aggrandizement was the key note for these dramatic displays, and descriptive details were included so that each incident, while belonging to a general category of war honors, was also peculiar in itself and recognizable as associated only with the warrior who had performed it. Only the older men, whose earlier exploits were well known and who had already assumed positions of tribal leadership, depended upon other types of achievement for the overt expression of their leadership. From such men, public opinion expected dignity and a wider and calmer viewpoint. The literature is replete with accounts of the major conflict in tribal affairs which was introduced under European contact because of the difference in role between the hot-headed young braves and the dignified older men. Europeans expected "chiefs" to control their warriors—an expectation not generally in keeping with Plains culture and constantly more difficult of attainment as the Plains tribes came more and more under the pressures of the contact situation.

Important as war honors were in the competition for personal prestige and social rewards within the tribe, they were almost equally important in intertribal affairs. When armed forces were approximately equal in strength, so that the result of combat was not a foregone conclusion, they often resorted to "sham battles," as the literature calls them, to settle accounts. For instance, Denig (1928, p. 550) tells of large forces drawn up in battle array, before which first one side and then the other sent a single warrior to parade between the lines singing his songs and calling out his names—recounting his war honors. These warriors boasted of personal and tribal supremacy over their enemies, and the competition played between them in these terms often acted to satisfy the war parties so that no general conflict was undertaken for the time being. Sometimes, the men got away with their challenge, sometimes several were killed, but the acceptance of these performances as action for the whole group was part of the general pattern.

Similar exhibitions of prowess, though robbed of the threat of immediate danger, were given whenever groups met. Before peace parleys, at chance encounters, during meetings for trade, men donned their war paraphernalia and reenacted their military exploits. The guests or hosts, as the case might be, were insulted. The individual and his group were aggrandized. These "war dances" were invariably interpreted by Europeans as inciting to immediate violence. Sometimes, in the late period, they may have done so. Overwhelmingly, however, the evidence shows that they set the stage for intergroup relations, and the group which came off second best in these competitive dramatics was incited to later war deeds of the accepted variety to use in later displays.

Social leadership was determined by personal competition in many parts of the Americas, and the amount of aggressive behavior tolerated in these situations varied sharply from one area to the other. Definite limits to personal animus were established in the song contests of the Eskimo, the potlatch contests of the northern northwest Coast, and the challenge contests to the Coast Salish. Personal supremacy was achieved by the person who kept within the definitions of these contests as set by the particular groups involved. The contests were staged for the maximum of popular participation and the rivalries were not idle —group disputes were settled and group leaderships were determined. The ball game of the Southeast, and of Mexico and Central and South America, was also used to settle group disputes or determine group leadership. It is in this framework of individual exploits, telescoped into a system of culturally accepted counters and used for determining personal status and group supremacy, that warfare on the Plains seems often to have been referred to as a "game."

Although all the counters varied, shifting from military prowess to success in the production and manipulation of property—from war to economics—the potlatch contests of the Northwest Coast can be seen to be almost directly parallel to Plains contests. Both cultures were highly individualistic in their focus, strongly emphasized the open elements of aggression, and built up the contrast between boasting and insult. Whereas, however, the Plains had an opposite ideal of dignified manhood and a ceremonial life emphasizing health, long life, and tribal unity (did, indeed, capitalize throughout on the difference between peace and war), the Northwest Coast suffered no such major dichotomy. Once the young man was absorbed into the potlatch system, his development was set in the same terms till death. Northwest Coast secret societies, which could actually have functioned as the kind of age-grading societies that helped transfer individual behavior from one type of role to the other on the Plains, did function hardly at all in this way. Membership in them served mainly to assist in the competition. Another, historical, contrast should also be noted, and its importance should not be underestimated: whereas European contact served to imperil the Plains pattern of social contest, introducing new factors and tending to weaken others, European contacts with the Northwest Coast reinforced the basic cultural drives toward economic motivations for prestige and furnished new wealth with which to play the old game.

Open aggression was less strikingly characteristic of Coast Salish challenge contests and Eskimo song contests. Other aspects of their social contests, however, correspond to those of the Plains and Northwest Coast. The Eskimo are often used as a classic example of people without warfare. Certainly, war and organized combat were foreign to them. It should be remembered, however, that, as individuals, the Eskimo sometimes exhibited a beserk blood-thirstiness unequalled on the continent. All along the boundary between Eskimo and Indian territory from the Mackenzie to Hudson's Bay, Eskimoes showed unbridled ferocity against Indian war parties and family groups. It was the social situation —the one involving familial or cross-familial groups—which demanded an absence of physical violence. The song contest worked excellently within its own setting, but, faced with the Indian war pattern, Eskimo frustrations seem to have mounted to the breaking point and exploded into uncontrolled violence. The ceremonial challenge contests which occurred from the Columbia to the Fraser were carried on by the Coast Salish in an atmosphere of "peacefulness" more reminiscent of the Eskimo than of their northern neighbors on the Northwest Coast.* The contests were marked by audience participation and a strong sense of group cooperation with the competitors through a pooling of individual powers. Data on the extent to which these contests settled social disputes and

* In this, the Coast Salish are also more like their eastern neighbors of the Plateau than like their northern neighbors. Plateau peoples, except where under Plains influence, seem to have been almost as lacking in organized warfare as the Eskimo (Ray, 1939).

provided for shifts in leadership are particularly full. There is an equal amount of material on the open aggression which took place outside of these controlled situations and which was used for more personal ends.

The patterned emphasis of the areas mentioned—Eskimo, Northwest Coast, and Plains—upon the individual has done much to conceal the group nature of their social contests. In each case, however, the activity which placed the individual in a position to win his personal counters for status involved a group of persons. Eskimo ceremonial and song contests were apparently part of a group ceremonial; Plains war honors were most frequently, though not invariably, won by a man while he was a member of a war party to which other warriors also belonged, and his training was always received in such a group; the classic Coast Salish ceremonial gamble was a group affair in which the competing leaders were backed in play and in supernatural aid by their village mates; and the potlatch of the northern Northwest Coast was made possible only by the close economic cooperation of village and kinship groups.

Another area of North America—the Southeast—strongly emphasized the group nature of its social contest. Here, the ball game was played between teams of men. Haas (1940) has clearly pointed out the important role played by these ball games in settling social disputes among the various towns of the Creek Confederacy. Throughout the Southeast, intratribal disputes might be settled by ball games of various sorts (Stern, T., 1950, p. 97; *etc.*). Intertribal affairs, however, were often settled, as among the Iroquois, by war or the threat of war. Warfare in these cases was intentionally punitive and destructive of property as well as of life, differing markedly in this respect from Plains warfare, and was characterized by the presence of a torturing complex* absent from other areas of North America. In Mexico, where tribute was exacted from conquered enemies, only the most recalcitrant foes were annihilated, and punitive measures were aimed largely at the collection of greater tribute.

In his recent review of rubber-ball games which occur in South and Central America, Mexico and southwestern regions of the United States, Stern (1950, p. 96) concludes: "It is no accident that strong parallels exist between warfare patterns and those of the competitive ball game. . . . The pitting of teams from two communities against each other in a game in which hard-driving, dextrous action wins high stakes, frequently though not everywhere, at the risk of injury or death, all lead to the occasional substitution of the game for overt warfare." This conclusion seems to imply that there was a self-conscious substitution of one form of socially recognized conflict for another or, at least, that true warfare chronologically preceded the ball game as a means of settling social issues. This is a conclusion of some significance, especially in view of our present world concern with conflict and the settlement of disputes, and will be reserved for fuller consideration at the end of this paper.

Plains warfare, especially in the late period, was tied to many of the group pressures which are strong in modern war. It never lost its role, however, as a "game" by means of which the individual acquired his personal status points. The Blackfoot, for instance, were engaged in a struggle to maintain their hunting territories against the westward pressure of Assiniboin and Cree. Yet a combined force numbering 1500 men from the Blackfoot, Bloods, Sarcee, Gros Ventre, and Piegan which set out after the Assiniboin and Cree stopped fighting after killing nineteen Cree. The Blood lost three men and retired after taking a few scalps. The Blackfoot, who bore the "brunt of the battle," lost six men. Kane adds: "The Blackfeet and their allies would immediately return to their own country after meeting with any success, and having a few scalps to take home, according to the war custom of the Indians" (Kane, 1859, pp. 417–419, 430–431).

Dead men had nothing to gain from the

* For a summary of this complex see Knowles, 1940.

social contest. Living competitors, however, were driven to greater and greater feats of daring. Warriors hampered their effectiveness in battle for greater ultimate gain in status: fastened to a stake by a two-foot thong in the midst of the fray, a man might make his reputation—or he might lose his life. As in the potlatch contests of the Northwest Coast, men were tried to the full measure of their endurance. And who could count on the chances of war? Eight hundred Blackfoot chased a party of fifty-two Assiniboin who finally took the last stand and killed thirty-four Blackfoot before they were themselves slain (Denig, 1928, p. 550). Colorful and exciting as it was, "the game" of Plains warfare was nothing to be entered lightly. From many points of view, the United States soldier today stands to lose more by combat than he gains. In the Plains, this situation was reversed and serious armed combat was combined with the euphoric anticipation of winning major social rewards by personal prowess.

Mourning-War. The close relationship existing among the Osage and Kansas between mourning and war practices was early recognized by J. O. Dorsey (1884, 1885), and G. A. Dorsey, in discussing the Osage (1902), hyphenated the two words, "mourning-war," to form the phrase which it has since seemed convenient to retain. Mourning-war rites are reported from the Osage in considerable detail from 1827 to 1928 (Smith, 1938, pp. 455–460). During this hundred years, actual warfare changed from a grim reality to a mere memory, but the relationship between death and ritual death at the hands of a member of a war party persevered. The ceremonial aspects of the Plains complex are most clearly marked among the village tribes of the Missouri. It is identifiable, however, throughout the area and can be traced widely through the Americas.

Literature on the Plains often speaks of war for "revenge." And again the meaning of a term used in describing Plains warfare has to be carefully examined. "Revenge" may suggest a reciprocity of action: a situation in which the perpetrator of the original action is himself acted against, and extension of the revenge to other individuals involves only the joint responsibility of the group to which he belongs. In extensions of this sort, where mechanisms for settlement are weak or lacking, the situation develops into "feud." Often, it is implicit that the original act was anti-social, even criminal, and feuds have, consequently, a legalistic tinge. They serve as mechanisms for righting the balance of sanctions in the society, and the reprisal is seen as justified, in view of the fact that it reestablishes the validity of customs which had been violated. The practice of assassinating a shaman whose behavior has become a social menace is rather widespread in some areas of the Americas, but this is done with such wholesale approval that no feud develops. The total society, in these cases, revenges itself against the miscreant. True feud, in its threat of continued violence between particular groups, is surprisingly rare in the New World. It can be truly demonstrated only for certain regions of South America and for California. Plains "revenge" must be recognized as a somewhat different syndrome of traits.

On the Plains, the death to be avenged might be any death. Not only those killed in battle but also those who died of disease or accident were avenged. Grief at the loss of a dear one took the form of excessive violence against the self. A mourner cut deep gashes in legs and arms, and amputated finger joints. In some cases, of course, these mourning practices were routine and semi-ritualistic. Nonetheless, the people certainly thought of deep grief as arousing a violence which was immediately self-directed, and war was equally recognized as the means for converting these self-destructive impulses into other social channels. Violence against other members of the tribe was traditionally tabu as running counter to true manliness.* Ag-

* When liquor was introduced, Plains warriors were famous for their murderous attacks upon each other, but such behavior had no sanction and was possible only when they were drunk. With the decline of war, intratribal violence has apparently

gression against an enemy, on the other hand, was fully approved. Male mourners, therefore, organized war parties or joined up with groups already planned. Female mourners were dependent upon their male relatives for a similar opportunity for releasing pent-up emotion and the return of a successful war party was the signal for mourning women to give way to an orgy of violence against enemy body parts and scalps turned over to them. Mutilation of enemy dead was common, but it should be particularly noted that no torture was involved in these outbursts. The displays were, or were made to seem, spontaneous, both on the part of men in the heat of battle and of women at the scalp dances, as were the first excesses of grief turned against their own bodies. Mourning ended with these excesses.

Feelings of "revenge" can be seen under these circumstances to be less involved with simple retaliation than with mechanisms for the reestablishment of a threatened personal prestige. This comes out very clearly in an instance cited for the Mandan: The wife of a distinguished war leader ran away with another man with whom he had previously been a rival for prestige. This not only shamed the leader but deprived him of his wife's help in certain ceremonies he had pledged himself to give. He went out alone, killed and scalped a young Assiniboin, and rode home on one of the young man's horses. A scalp dance was held during which he called his former wife to him, dressed her in the best clothing he could get, placed her on the horse he had taken, and sent her to her new husband's lodge. In recounting his deed (the highest honor in Mandan war honors was to go out alone and kill and scalp an enemy), he further shamed his wife by telling how he had spared the life of the Assiniboin's wife out of respect for his own motherless children. Not only did he thus shame his wife and eclipse his rival, but a family with two eligible daughters invited him to marry them so that he also had the help to perform the ceremonies he had pledged (Bowers, 1950, pp. 70–72). In reporting this war incident, the ethnographer says it "was for revenge." In a sense, this is true, but the emphasis certainly lay in a reaffirmation of an imperiled status position rather than in direct reprisal, and the Assiniboin who was killed was perfectly incidental to the whole affair.

It is necessary in discussing warfare, then, to distinguish between revenge in which the perpetrator of a crime is himself killed, possibly along with the group to which he belongs, and revenge of the kind just cited, which is against one's fellow tribesmen and involves, as a sort of intermediary, the killing of a stranger who had nothing to do with the original incident. The latter forms an important part of the revenge present in the mourning-war complex, whereas it would be meaningless in the simple feud situation.

Among the village tribes of the Plains, the ceremonial aspects of mourning-war were highly developed. Scalps or portions of scalps were placed on graves as formal symbols of the end of mourning. These did not have to be the scalps of people responsible for the death of the person whose grave they decked. In the late period, they might even be chicken scalps. In any event, however, the scalp formed a necessary link in the mourning-violence reaction chain. When enemies were beheaded, as by the Osage, it was apparently done in order to obtain a larger head skin at greater leisure. Wherever it was taken, the scalp was ritually treated, not only by processes which served also to preserve it, such as smoking, stretching, and coating it with ochre, but also by acts which could have no such function. In the village tribes, the scalp was, for instance, adopted as a member of the tribe, addressed by kinship terms and kept with the gens bundles. There was an explicit effort to replace the deceased by the scalp and, among peoples like the Mandan, the reciprocal moieties had major roles in the scalp dance.

This use of war or homicide as a kind of

increased. It is also worth noting that many of the bravest deeds cited by the Plains tribes, such as various Crazy Dog vows, could easily be thought of as incidents of socially sanctioned, and honor-deriving, suicide.

ritual death allaying mourning and associated with kinship groups has interesting distributional ties. It is strong among the Southern Sioux, as we have seen, and Swanton (1928 b, p. 718) notes it as one of the traits which occurs in the northeastern section of the Southeast, in Algonkian and Siouan territories, and on or near the lower Mississippi, but not between. Among the Eastern Sioux, the persons killed to "accompany the soul" of a dead man were enemies obtained in war. Among the Natchez, the persons killed were the deceased's friends and relatives. Wherever the scalp was taken and the scalp dance occurred, *i.e.*, Plains, Eastern Woodlands, Southeast, and Southwest,* there are sporadic reports of the scalp being "adopted" into the tribe taking it. In the northern Plains and eastward through the Iroquois, the scalp was insulted and reviled. In more southerly areas, it was as often "danced" and honored. The prisoner of war was adopted by the Iroquois before his torture, often by an old woman as her "son," and she took a leading role in the finer aspects of the torturing art. Sahagun (1932, p. 77) reports that the Aztecs adopted prisoners of war before they ate them, calling them by relationship terms. Furthermore, there are well-documented cases of prisoners of war being honored by the Aztecs, even to the point of impersonating gods, before they were ritually killed.

Traits apparently associated with this ritual complex include flaying, decapitation (or the two combined), particular attention to the heart of an enemy, and cannibalism. Burning as an honor in torture, which occurred in the true torturing areas of the eastern seaboard of North America, is difficult to ascribe, because burning was so obvious a trait in the European punitive pattern and contact with these tribes was established so early. There can be no doubt, however, that European influence tended to increase the number of body parts taken, since European nations regularly offered bounties for trophies to their Indian allies in the early wars along the seaboard. The Pawnee, alone among Plains tribes, engaged in ritual torture, and it is noteworthy that this was coupled with what seems to have been an impersonation of a diety (Linton, 1926). With this exception, torture on the Plains was entirely self-torture, engaged in to obtain supernatural favors or during the crazed grief of mourning. The former, as in the sun dance, was carried through with a minimum of emotional expression. The supernaturals were asked to pity the sufferer, but there was no dramatic build-up of emotional display for the benefit of human observers. The latter capitalized upon dramatic display and translated readily, as we have seen, into scalping and mutilation of enemy dead. But, whereas scalping seems to be an important part of the northern distribution of the ceremonial aspects of mourning war, mutilation of the dead probably cannot be so considered. Insults perpetrated against the bodies of the foe were here expected only to arouse horror and fear, thus establishing the supremacy of the victors.

On the Northwest Coast, similar insults, beheading, *etc.*, were further designed so to impress the enemy that future forays would be discouraged. A recent study of Kwakiutl warfare has pointed to the fact that the emotional transition from mourning to war, in the more intricate meaning of revenge outlined above, was similar to that of the Plains (Codere, 1950, pp. 106–107). A mishap to a Kwakiutl or to a member of his family aroused "grief," "anger," and "shame," which were allayed by violent aggression against an enemy. Kwakiutl potlatching activities were also frequently touched off by mishaps to the individual or to members of his family, so that the sequence from being hurt or harmed to being aggressive recurs in this realm of culture as well. The same sequence can be traced elsewhere on the coast, but one additional

* Scalps were also taken in parts of central California, and a scalp dance was held in which the enemy were insulted through taunts and actions directed against the scalp, the scalp effigy, or severed body parts. In these cases, no mourning-war sequence seems involved, and it is probable that we have an instance of the adoption of a visible trait minus its cultural implications. It might also be added that some authors have suggested on the basis of Woodlands materials that scalping is a recent New World phenomenon, but the North American data given in the paragraphs above seem to me to argue quite effectively for its considerable antiquity. Further mention of this fact is made in the text below.

instance must suffice. Among the Lummi, a man started out on a raid "as a protest against some wrong done in the past or to avenge the death of one of his relatives. . . . Some men join the war party because of slights at potlatches . . . some because they wish release for their pent-up feelings occasioned by mourning. . . ." (Stern, B., 1934, p. 97).* Interesting as this parallel is, however, it should be pointed out that all the more ceremonial aspects of mourning-war seem to have been lacking among the tribes of the Northwest Coast.

A mourning-war complex is not easy to pin down. The distribution of traits suggests that it is old in the Americas and that the taking of the scalp is a later development out of earlier decapitation and flaying. Yet even this development must be dated well into pre-Columbian times. A partial trait list, which I drew up some years ago, numbers several hundred items. Although a full study of these might well add considerably to our chances for a full historical reconstruction, that is not now possible. The brief discussion given here does suggest, however, that the complex as described earlier falls naturally into two parts: first, the set of "emotional" reactions, and related traits, but through which the individual who had suffered hurt directed "aggression" beyond the circle of his friends and relatives and, if successful, reestablished himself in his own eyes and in the opinion of his society; and, second, the traits through which the individual, as the member of a group, related social mourning and loss to sanctioned homicide. The organizational aspects of this second, ceremonial phase of mourning-war brings us to consideration of the war party.

The War Party. Everywhere in the Americas, except for some portions of the Andean highlands, participation in war was entirely voluntary. Great social pressures might be brought to bear to make a man go to war but, in the last analysis, an unfavorable dream or a premonition of bad luck could cause him to withdraw without loss of prestige. It does not follow from the voluntary nature of its membership, however, that the true war party was loosely organized. Once underway, the constituent roles and the course of action were both prescribed (Smith, 1938, pp. 434–440).

War party leadership was likewise voluntary. Any previously successful warrior could and did organize a war party. The war leader, however, wielded an authority for the duration of the party which was most unusual, since American Indian political situations seldom allowed any type of enforceable authority to leaders. In some areas, as along the Missouri and in the Southeast, where moieties were often referred to as "peace" or "war" groups, the distinction seems to have been based more upon a demand for ceremonial completeness than any real military distinctions between their respective personnel. Warriors and war leaders came from either moiety. Similarly, the dual chieftainship of the Plains, the Southeast, and parts of Mexico, phrased as it was in terms of "war" and "peace," did not give sole prerogatives of actually waging war to the "war chief." Only in some tribes of the Southwest and along the lower Colorado was a single man given functions in war leadership, and this was consistently because of his special powers, derived either from ritual knowledge or supernatural experiences, for dealing with the impurity of enemy contact. Even here, however, the actual authority of leadership during a campaign was vested in men who had demonstrated their ability in war.† There was no compromising with the unskillful and the unlucky. War leaders had to be good or they had no following. Furthermore, although the prestige of the leader carried over into everyday affairs, his authority did not. It began when a particular war party left its home village and ceased when it returned.

* Among other reasons for giving a potlatch, Frederica de Laguna mentions for the Tlingit "the reaffirmation of a social status which has been jeopardized" (ms. "Some Dynamic Forces in Tlingit Society"). The parallel in this phrasing and that used in this paper is striking.

† It is extremely interesting that the lower Colorado tribes which were so spectacularly warlike should, in these regards, be so similar to the Pueblos who, in their internal affairs, laid such stress on virtues not expressed in warfare.

On the Plains, the war leader himself usually had to obtain the special supernatural power traditionally necessary before a war party could be organized. Without such a power demonstration, in addition to his personal warrior powers, he was not fully equipped for leadership. Each party needed its own supernatural sanction or assurance of success. The man of proven ability, with this special assurance, is often called a "partisan" in the literature on the Plains, and it was as such that he figured in war party ceremonial. Sometimes in the Plains, and very generally in the Southeast and Southwest, the man who furnished the supernatural validation for a party was not the leader. In the Southeast and Southwest, he did not even have to be a member of the fighting strength of the party, and, in the Southwest, he often did not accompany it.*

In the Southeast, parties were regularly attended by temple priests and the line of march was preceded by the "ark" from the temple, *i.e.*, in less archaic terms, the tribal or group medicine bundle-housed in the temple. Such temple and priestly organizations were, of course, absent from the Plains, but a reflection of the practice may be seen there in the distinction which was made in several tribes between more formal bundle parties and the usually smaller, and simpler, ones unaccompanied by special medicine bundles.

Scouts were widely employed by war parties. Their function was not only to guard against surprise but, when enemy territory was reached, to keep a constant lookout for possible foes. Once the party had attacked, it had to return home. It could not proceed after a kill had been made, or an assault or theft perpetrated. If, then, a large party met a lone enemy, the whole venture was over. It was up to the scouts to inform the leader of all human activity and all signs of human activity within several miles of the party. The leader could then judge the direction of march. The party could not retrace its steps and it could not make a day's full halt. But the direction and length of each day's march were variable and were largely decided by the leader.

War parties aimed to make surprise attacks. Plains "sham battles," of which mention was made above, generally occurred only when war parties from different tribes happened to meet in the same territory. The usual attack aimed to strike at dawn and get away again before the enemy had a chance to organize for resistance or pursuit. Because of the danger of pursuit, it was not advisable to get too deep into enemy territory. During the historic period on the Plains, many parties went out on foot—it was difficult to move with the necessary secrecy with horses—and counted on obtaining mounts from the enemy for a getaway. Mounted parties often left their horses at some distance from the encampment or village to be attacked, planning to pick them up on the return. Even in these cases, they rode off on enemy horses whenever possible, both as a means of quick escape and to deprive possible pursuers of mounts. Also to prevent pursuit, scouts were often assigned to cut loose, or disperse, any horses which could not be taken with the retiring party. The most spectacular, and hence the most bragged about, attacks were those which could be made by mounted warriors against a sleeping foe. The final responsibility for all such decisions, however, lay with the leader and naturally depended to a great extent upon immediate circumstances. Warfare against White settlers could relax many of the precautions necessary for intertribal warfare. But, when Plains warriors met the military strength of trained troops in the nineteenth century, new rules and new conditions had to be faced. The courage and intelligence with which they did so is constantly commented upon in the literature. Many of the conditions of actual fighting changed,

* In central California, chiefs attended the fights and arranged for their start and finish, but did not themselves have to participate. This is reminiscent of general southern United States practice. But California is clearly outside of the true war party area, and I am inclined to think that the organization of California fighting forces followed the general Pacific pattern of a group backing up its leader, in a manner not unlike the cooperation given leaders in the northern social contests.

but, to the end, the Indian relied greatly upon his scouts and his skill in the surprise attack.

Raids against sedentary villages could be planned with greater ease and executed with greater surety of success than those directed against more shifting populations. After small-pox had decimated the village tribes of the Missouri, and fear of pursuit and reprisal from them was consequently lessened, they seem to have been peculiarly singled out for attack. Bowers's census for 1870–1872 (1950, p. 82) among the Mandan shows that males constituted only 38 per cent of the population, and this figure includes boy children: "Plains warfare greatly unbalanced the proportion of men to women." Apparently, the Mandan went down fighting. The Pawnee, who were attempting to live peaceably under the contact situation, were correspondingly open to war party tactics and suffered a 25 per cent loss in population at the hands of the Sioux in four years (Lesser, 1933, p. 15). This loss was pretty evenly distributed between the sexes. It should be noted in this connection that everywhere in the war party area the warrior received equal honor for killing and scalping either a man or a woman. Coup, however, was ordinarily counted on a man, and it may be that coup counting, and certain deeds of spectacular valor, increased with the increase of combat between war parties and the influence of European forms of fighting.* It might even have been preferable, with such ends in view, for the leader to meet another war party than to make a surprise attack upon an encampment.

In the Southeast, where sedentary villages were the rule, and fortifications were often associated with them, attacks frequently developed into lengthy engagements and ended with the total destruction of the village or the complete rout of the attackers. The same held for the various Iroquois groups. The horse was not a part of war accoutrement in either area and the mobility of troops was consequently much less than on the Plains in the historic period. War party tactics were otherwise similar but they often took on a cumbersome and, at the same time, a decisive character which we find easier to associate with the movements of true armies. In both of these regions, also, war was formally declared against particular peoples and marked the breakdown of previous negotiations. Peace was also formally arranged, by parley, and signalized by an exchange of gifts.

Obviously, a political organization with fairly defined powers for speaking for the group lay back of such customs. Although similar ideas existed on the Plains as to the distinction between a state of war and a state of peace, it had no such reference to whole groups. On the Plains, membership in a war party constituted being "at war" for the individuals involved. The rest of the tribe was not necessarily involved and the state of war ceased with the return of the party. Although parties customarily went in certain directions and certain tribes were traditional enemies because of long histories of attack and reprisal, there was no state of war which prevented the groups from meeting for other purposes. There was, also, no state of peace between groups which could adequately guarantee against attacks. The pipe of peace was a ceremonial prerequisite to parley, but, in the last analysis, it committed neither side to anything. Gradually, the Plains tribes seem to have learned the value of coordinated tribal action in peace and war. Individual leaders early pleaded for a common effort to meet the threat of the White invader, but it was not until late in the nineteenth century that whole groups could be brought to coordinate, and sustain, their efforts. The Iroquois and the tribes of the Southeast, however, despite various schisms in their ranks and despite the ever present possibility of individually organized parties, seem always to have been capable of such action.

The apparent differences between Plains and eastern warfare should not blind us to

* This is the kind of point on which careful checking of sources would throw tremendous light. Many questions in regard to American Indian warfare need similar illumination.

the fact, however, that their underlying aspects were similar. As Swanton (1928a, p. 405) has summarized it: "the truth of the matter was that war among the southern Indians was a social institution and war-like exploits necessary means of social advancement."

Nowhere are the ties between Plains and eastern warfare more obvious than in what might be called certain "non-essentials" of war party organization. Assistants to the war leader were appointed by him, and assistants to him in his role as partisan, or to the man bearing the supernatural validation of the party, were likewise appointed. The former are often referred to in the literature as "lieutenants" and served generally utilitarian functions. The latter, however, who are frequently called "kettle-bearers," were mainly important as ceremonial watch-dogs. Although they often tended to any cooking which was done while the party was out (rations were generally in the form of concentrated and dehydrated foods eaten cold and were supplied by the warriors themselves), their responsibility lay in seeing that none of the eating and drinking tabus were broken. Rules of sexual abstinence were also placed upon warriors. In areas such as these, where tabus were not usual and were only associated with the obtaining of individual guardian spirits and the maintenance of medicine bundles, the very presence of any tabus at all is worthy of comment. The persistence with which they are reported serves to underline the relationship felt to exist between warfare and a peculiar attunement to supernatural forces. The duration of the war path marked this special "attunement" and explains the fact that only one aggressive attack was possible to each war party. The road of war had to be set apart from more everyday behaviors. It began with ceremonies at the first camp, continued through various tabus on the line of march, reached a climax in the attack, and culminated in the scalp dance (Smith, 1938, pp. 440–447).

In the whole of the Southwest, from the Rio Grande to the lower Colorado, the dangerous, extraordinary nature of warfare was so emphasized that much of war party ritual concerned itself with preparation for, and final purification from, its powerful influences. Scalp rituals particularly were designed to shift the possible malevolence of the scalp into the desired benevolence. The keeper of the scalp held a delicate situation in balance, and the warrior who took the scalp assumed, after his necessary purification,* special posts for which peculiar powers were needed. Warfare in the Southwest had a kind of ambivalent quality quite out of keeping with attitudes on the Plains and in the east.

Further south, among the Aztec, warfare also had two aspects. The ceremonies of war had elaborately horrific elements involving human sacrifice of prisoners on a massive scale, yet these were looked upon as essential to the continuance of the pleasanter portions of existence, such as good crops and fertility, and found their counterpart in other rituals which were mild and benign. These aspects were never quite distinct, and "the ritualistic conception of war as the earthly re-enactment of the titanic struggle between opposing forces in nature has a quality almost sublime . . . a celestial antiphony in the Sacred War which the Sun fights each day as he, by his own death and sacrifice, ensures the life of man. The War of Flowers was undertaken to satisfy this yearning when no active campaign was in progress. In this incongruously named ceremonial combat, the best warriors from several states met in a very real battle, so that feats of arms could be accomplished and captives taken to satisfy the hunger of the gods. . . . Such warfare has no place in a conflict with Europeans" (Vaillant, 1941, p. 22). It can be seen that the presence of a true pantheon, lacking in northern North America, led to many apparent discrepancies from northern practice. Yet, once one substitutes the taking of a prisoner for a scalp or other war

* On the Pacific coast, the murderer might have to purify himself, but generally the warrior did not.

honor, and examines the actual organization of the Aztec fighting force, its tactics, and its culmination in a triumphal return, correspondences with northern areas seem indicated (see particularly Bandelier, 1877).

In 1941, I pointed to the close correlation which exists in the New World between the northern distribution of agriculture and the distribution of a discernible war complex.* Although many of the data we would wish to have are no longer obtainable, it is still possible to map the outlines of a distinctive set of war patterns. It is striking that these correspond almost exactly to the northern distribution of maize agriculture. Only on the western fringes of the Plains can one trace somewhat similar war patterns among non-agricultural peoples, and these are demonstrably post-Columbian in date. After the more careful analysis of this paper, it seems more accurate to describe these North American war practices in terms of two complexes and to extend somewhat the area covered.

What has been previously referred to in this paper as the "ceremonial" aspect of mourning-war seems closely related to the organization of the war party and the concept of the warpath. Together, these form an elaborate socio-religious complex relating individual "emotion" to social reintegration through group activity and sanctioned homicide. As we have seen, there are notable variations within this complex. But it embraces all of North America east of the Rockies and south of central Canada, and may be referred to conveniently by the old term "mourning-war." A new term is then needed to indicate the socially simpler sequence of behaviors, in which the individual acts relatively independently to reestablish his status, or his view of himself. This overlaps on the northern part of the "mourning-war" complex (as newly defined); has an extension beyond the Rockies to the Pacific Coast;† and may be thought, in one sense at least, to "underlie" the more elaborate complex. The somewhat unsatisfactory descriptive phrase, "shame-aggression," is suggested for this simpler complex.

Causes of War. It has been necessary to elaborate upon the socio-religious aspects of American Indian warfare because they are foreign to European patterns and, consequently, not easily understood. Once they are clear, or relatively so, it seems to me impossible to deny that the wish for their fulfillment furnished one of the "motivations" for war.‡ Yet, to over-emphasize these socio-religious elements would do slight justice to the Indian and would place him in a category so outside our own military tradition as not to be warranted by the events of European contact. In areas where the socio-religious elements in war were strong, the Indian still made an excellent fighting man. After the first shock of armed combat, he made quick adaptations to new weapons and new tactics. The "Aztecs put up one of the most desperate defenses of history" (Vaillant *op. cit.*). And warriors of North America successfully competed on the military level with some of the highest ranking generals of the Civil War.

Starting with a definition of war phrased in terms of "competition for anything that is valued" and laying particular emphasis upon economics, Newcomb (1950, p. 317) has called attention again to certain important aspects of Plains warfare under the contact situation. He concludes "that the causes of warfare are to be found in the

* Paper given before the annual meeting of the American Anthropological Association, Philadelphia, December, 1941.

† The nomadic hunting tribes of the central Plains are now recognized as having had a previous history as semi-agriculturalists. The Cheyenne, of course, furnish the best example of this transformation in basic economy. Tribes of the northern Plains, particularly the Blackfoot, seem to belong as much in one of the mourning-war distributions mentioned above as in the other. For this and other reasons not possible to include here, the Blackfoot should probably be thought of in relation to an early distribution of traits tying them with the area west of the Rockies. This distribution seems also to carry east beyond the Blackfoot. Although certain Iroquois war traits seem to relate them to the Southeast, many central Woodlands tribes belong, like the Blackfoot, as much in the "shame-aggression" distribution as in the distribution of the more elaborate "mourning-war" complex.

‡ It is probably obvious from the preceding sections of this paper that I cannot hold with Newcomb's (1950) distinction between the "causes" and "motivations" of war. To distinguish between "sociocultural" and psychological "levels" of experience may be fruitful in some contexts. Applied to warfare, they seem to me to set up categories based mainly on our own ethnocultural standards and the convenience of traditional academic disciplines. When data show the "levels" to function simultaneously, it seems misleading, as well as artificial, to adhere to them.

migration and displacement of tribes, in the competition for good hunting grounds, the need for horses and weapons, and in the machinations of traders" (*ibid.*, p. 329). Jablow's (1951) thorough analysis of these factors, however, especially the need for horses, weapons, and trade goods, stresses the fact that they had an equally significant effectiveness in just the opposite direction. Peaceable trade relations and diplomacy furthering concerted intertribal policies also increased. It seems certain that there was a total acceleration of Plains culture after the introduction of the horse and the increasing absorption of the tribes into the economy of the fur trade. Both warfare and non-hostile relations were apparently affected and intensified.

The whole question of the relationship between economics and warfare needs attention. It is perilously easy for scholars of the mid-twentieth century to perceive and appreciate economic factors in warfare. After several cultural periods during which European warfare was validated on essentially non-economic grounds, we feel a glow of revealed truth about seeing through the smoke screen to the "real" causes of war. So much of present-day armed conflict is closely connected with economic struggles for survival and dominance that our faculties for discerning economic "motivations" have been highly sensitized. To deny the significance of economic factors in warfare would be patently absurd today. It would, furthermore, violate every anthropological tenet on the integration of culture. The question of prime concern, therefore, in any analysis of the relationships between economics and warfare becomes less a matter of whether such relationships exist than an investigation of the peculiar nature of their interaction in any given situation.

Some areas of America made an immediate and direct connection between "anything valued" and the act of seizing it. The simple logic of taking what one wanted was hampered only by consideration of whether or not one could get away with it. On the Northwest Coast, this is clearly seen in Coast Salish and Nootka (Smith, 1940, pp. 150–161; Swadesh, 1948). Among the Coast Salish, organized warfare was poorly developed; among the Nootka, it was fairly efficiently arranged. The Nootka fought for the possession of certain property rights in land and fishing sites which were not points of issue among the Coast Salish. In both groups, however, a considerable part of warfare activity was directly motivated by the simple seizure of goods which were immediately, and of themselves, of economic value. Consumer items might, in these cultures, be obtained by overt aggression, and echoes of this practice can be discovered among other Northwest Coast groups as well. Among the Kwakiutl, where intangible property, such as the right to names, crests, dances, *etc.*, had such exaggerated prestige value, a similarly direct mode of obtaining it by killing its previous owner seems once to have been widespread. Such seizure had later to be validated by the usual expenditure of property at a potlatch, but the economic goods so used in validation were earned, not taken in war.

Codere (1950, p. 105) has pointed out that, in the more usual understanding of "economics": "It is impossible to discover in Kwakiutl warfare an underlying economic motivation . . . In Kwakiutl, unlike Nootka, . . . plunder does not figure at all or it is of minor significance . . . There is no instance in the Kwakiutl literature in which the purpose of war is to gain land or fishing rights . . ." Such a statement does not suggest that the Kwakiutl were not economically motivated. Far from it. But it does clarify the kind of differences which may exist in the integration of economic benefit and overt aggression. In the late period, the Kwakiutl leader who was driven to slaying his potlatch rival was himself, by that very act of over-simple solution to rivalrous competition, most ignominiously defeated and, despite the terrific economic drive toward production, seizure of the resources for production was equally, or even more effectively, ruled out.

All through the Northern Plains, espe-

cially among the Blackfoot, the importance of direct economic transaction was very great. Medicine bundles, which furnished personal, social, and ceremonial validation for most areas of cultural participation, were bought and sold. A medicine bundle captured in war, however, was valueless to the captor except as a token of supremacy. There was no direct recognition of the fact that simple seizure of a bundle constituted a transfer of ownership rights. To use it, one had to go through more elaborate social processes. The expenditure of property also obtained for the individual various intangible, but highly valued social approvals, such as those called for by kinship obligations. These, however, were not thought of as being purchased, and the goods expended for them were purchased as gifts. Throughout the Plains, with the possible exception of the Kiowa and Comanche, ownership of property at any one time seems to have been less important to the prestige of the "chief" than his general reputation for generosity. Prices and profits were carefully watched and economic advantage was of prime importance in matters of trade. In obtaining social prestige through property distribution, however, generosity won most acclaim and the grand gesture caught the public eye.

Property had, consequently, a social value even beyond that conceived in a strictly "economic" sense, and horses came, at least by the nineteenth century, to be the single item most often used in such transactions. They served as a kind of fluid standard of value in the more elaborate economic transactions of bundle transfer, and in the economic validations of various social approvals, as well as being employed as tools of production to obtain and transport greater quantities of skins, meat, *etc.* Plains war parties, which seem not generally to have taken consumer goods in significant quantity, went out for two avowed purposes: "for scalps" or "for horses." Purely mourning-war situations called for scalps; the ordinary brave with an interest in his own advancement could use any war deed to advantage; and horses could be used to obtain other social prerogatives. The Cheyenne and Crow, as noted above, counted the simple capture of horses as a war honor, and among the Cheyenne, where the horse was peculiarly important in the tribe's economic role as middleman in Plains trade, warriors sometimes simplified or avoided warpath ceremonial in order to facilitate the successful capture of horses (Jablow, 1951, pp. 69, 84; Grinnell, 1923, **2:** 2–3). Even in such cases, however, the war leader had to justify his action and explain away the ritual for the particular horse raid he was organizing. The force of the other "excuses" which had to be made by warriors to sanction homicide were still strong and the man who joined a horse raid still often said he was going for "revenge." In view of these and similar materials, it seems to me preferable not to stress a chronological transition *from* "socio-religious motivations," *to* "economic" ones. The factors seem to have coexisted, and at no period does one find in Plains accounts emphasis upon the simple logic expressed so naturally for the Southwest: after the first raid, "the war party went on for more booty" (Curtis, **17:** 112*).

European traders obviously had rights of safe conduct through eastern and central North America. The travels of such persons as Adair and the Henrys amply testify to this fact, and earlier accounts likewise indicate that only the priest and the roué were interfered with in their local pursuits among the Indians. Later, when the Plains tribes were making desperate bids to monopolize the benefits of European trade, the "pillaging the traders of their goods" was most frequently a kind of detention which resulted in the traders being glad to get away with low profits. The detention was maneuvered partially by false statements of the dangers to be encountered among more distant tribes, partially by the

* Sheep and horses were both included in Southwestern "booty." Horses were considered apart from such "booty" as was occasionally taken on the Plains and which included maize and food items. In both areas, "booty" seems to mean simple consumer items and the horse was certainly more than that on the Plains. All such economic points seem to need much more careful comparative treatment than they have as yet received.

introduction of a hundred annoying mishaps which interfered with the trader's onward journey, and partially by a show of physical superiority which intimidated the trader into staying and disposing of his goods at prices favorable to the Indians (Jablow, 1951, pp. 35–39*). Theft is sometimes, though rarely, reported, and I know of no incident such as that which occurred when a trader first entered Zuni. The Zuni calmly chucked him over a cliff and appropriated his goods.

This direct reaction to the thing "valued" can be demonstrated in many of the Southwestern materials. Even the taking of an enemy scalp seems often to have been motivated by a simple need to obtain the curative powers inherent in it. The southernmost Plains tribes, the Kiowa and Comanche, seem also to have been ready to recognize a perfectly simple economic reason for participating in slave raids. Yet war was not lightly undertaken, and the occasional, directly economic validations of the Southwest by no means replaced the more intricate and ceremonial validations which also existed there, and war practices themselves were closely knit with the total picture of Southwest ceremonialism.

Farther south, in the area of high culture, trade was a recognized institution with rights of safe conduct usually granted to traders. Many Mexican intertribal wars seem, moreover, to have been just as frankly economic in their validation as was trade. Such war was designed to seize booty or, more profitably, to increase the amount of tribute due the victors, and the treatment of defeated peoples was dictated by the need to keep up their productive capacity. Goods and more goods were desired, and the domination of neighboring peoples through armed combat seemed one obvious way of obtaining them. Other Mexican battles, as we have seen, were fought because of quite other factors. Mayan warfare, like Mexican, apparently had components of the social contest as well as various of the ceremonial and economic aspects already described. In the Andean highlands, manipulation of the productive capacity of conquered peoples reached a fine art among the Inca, almost totally eclipsing any purely hostile or destructive impulses in war behavior. In short, it is clear that impirialisms of special sorts, which deserve much more attention than can be given them here, developed in the high cultures of Middle America. That many of these were flagrantly "economic" seems abundantly evident.

Even such a brief survey of the economic reasons for war shows marked variations to have existed in the Americas. The arc of behavior seems to have lain between one extreme involving the simple seizure of the thing valued, occurring mainly in the Northwest and Middle America and less frequently in the Southwest, and another extreme in which the socio-religious "motivations" described above were almost wholly non-economic in character and the cultural demand for fulfilling them was such as to gloss over, or actually to rephrase and redefine, those economic factors which were present due to historical and other circumstances.† These socio-religious causes of war are typical of the entire mourning-war area from central Canada at least as far south as the Valley of Mexico.

In California, which lies outside the mourning-war area, a whole new set of causes for war is encountered: "... war principally served the purposes of revenge or punishment. In the northwestern subarea, the occasion was generally a legal default measurable in terms of property and obligation. Persons, families, and lineages were more likely to be involved than settlements or communities. For

* Recounting of war honors by tribes meeting for trade was, as mentioned above, usual Plains practice, so that the last of these modes for obtaining favorable prices fits directly into an old pattern.

† Further documentation of this lack of economic "motivation" in what to us might be defined as primarily economic situations is to be found in Flannery and Cooper's study of Gros Ventre gambling: "So far as we could make out, Gros Ventre attitudes on gambling were determined in only minor measure by economic considerations. . .Gros Ventre attitudes on gambling appear to have been influenced very much by concerns for altruism and accord, very little by economic values" (1946, pp. 417, 418).

the remainder of the state, it has been said that the cause was normally a death attributed to poisoning or bewitching. Within a community, shaman killings . . . were adjusted without further bloodshed . . . If suspicion fastened on a member of another community, a war might result . . . Among certain groups, war was evidently more frequently caused by poaching" (Kroeber, 1928, pp. 394–397). This is, as remarked above, a region having true feud, and the causes of conflict in California may be briefly summarized as threefold: witchcraft; the abduction of, or insult to, a woman; and land trespass or boundary disputes. In the last, where economic reasons are clearly involved, the actual situations, however, seem always to contain elements of insult and of real or imagined outrage as well. Although "the general tenor of life was unwarlike" (*ibid.*, p. 394), individuals were touchy and sensitive, and their reactions could spread over whole groups to create conflicts not unlike those existing in pitched battles. These actions were seldom decisive, however, and the whole atmosphere of hostility had a smoldering, non-culminating quality which may well be characteristic of feud areas.

It is probably important to the total picture of American Indian warfare that, although many of the traits of the war party and mourning-war recur in interior South America, as among the Jibaro, the causes of war in South America are generally similar to those of California. The sketchy materials from the Florida coast, which was so soon engulfed by the European invasion, are also reminiscent of the same practices. The ties between these remote regions are strengthened when one includes in the possible syndrome of traits a purely combat habit in which women accompanied the men to battle and occupied themselves by salvaging enemy arrows to replenish the gradually depleted supplies of their own fighting force. This trait occurs sporadically in North America, but always, so far as I know, in coastal or otherwise literally marginal areas.

Taking all our factors into consideration, four major sets of distributions seem to emerge: (1) the *feuding pattern* just referred to, which is apparently old and, judging from its distribution, may be judged to be pre-agricultural; (2) the area of *social contests*, in which many of the functions of war were performed by non-warlike (though often highly rivalrous) activities, spreading from the Arctic deep into South America and, from this distribution, judged to be old; (3) the area of the simpler aspects of mourning-war, or *shame-aggression*, probably to be tied to early manifestations of head-hunting or scalping (through which trait it may be inferred in many archaeological materials), and covering almost as wide an area as that of the social contests; and (4) the area in which the ritual aspects of mourning-war and the traits of the war party combine to form a *mourning-war* pattern which is strikingly correlated with the area of the northern distribution of maize agriculture. Beyond arranging these sets of distributions in tentative time sequence from 1 to 4, as given here, no more elaborate historical reconstructions seem possible at this time.

War Substitutes. Attention to another type of sequence may serve to conclude our survey. As indicated above, Stern has recently suggested that the ball games were "substitutes" for war. Such phraseology immediately raises the question of a possible developmental sequence. Were such "substitutes" self-consciously employed to replace, or decrease, destructive and warlike activities?

One careful survey of historical materials tends to answer our questions in the affirmative. Codere (1950) clearly shows that overt aggression directed against life and limb was gradually transformed among the Kwakiutl into an aggression equally overt, but not employed against the physical well-being of one's enemy. Fighting with weapons became, in the potlatch of the late period, fighting with property. If the Kwakiutl cannot be shown voluntarily to have planned such a change in their

social institutions, they were at least well aware of the fact that the change had taken place. On the other hand, Collins (1951) points out that the tempo of Kwakiutl warfare against the Coast Salish heightened appreciably in the contact period. This is the same time span dealt with by Codere. In handling this apparent contradiction, one should remember that potlatch aggressions could replace more openly destructive aggressions only within those groups belonging to the potlatch system. The Coast Salish were not in this system, and raids against them offered no reproach in the more highly valued potlatch rivalries. If in-group substitution of rivalry by a social institution other than war should be accompanied by an increase in out-group warfare, however, our "substitution" would be of little social value. Fortunately, this seems not to have been the case. The Kwakiutl-speaking Lekwiltok seem to have been the ones most involved in depredations against the Coast Salish. These same Lekwiltok are revealed by Codere's study as the most warlike of Kwakiutl groups, and their ranking in the potlatch system was never high. They seem, therefore, on several counts, to stand rather outside the development which occurred among more northern Kwakiutl.

That the social contest may have been, partially at least, a voluntary and intentional substitute for other types of aggression is suggested for the Creek. There, disputes between male children were directed into ball play and the competition* of the ball game was consciously substituted by their elders for the more direct aggressions of wrestling. Such training would certainly aid in adult acceptance of the ball game. Only further, detailed studies can be depended upon to answer reliably the question regarding war substitutes in the Americas. It is important, however, to recognize that various social mechanisms other than war existed in the New World by which status and prestige were acquired, social issues determined, and rivalries given free rein.

* Personal communication from Mary Haas.

If American Indian data, as known at the present, carry any valid comment on war and substitutes for war, it must be in terms of the fact that, when warfare was concerned with political domination and personal or group aggrandizement, its destructive aspects seem to have been elaborately heightened, except in those regions where the social contest remained of major importance. On the other hand, when competition and rivalry became most frankly economic, warfare itself became less destructive of life and property. Apparently, destructive warfare could not, in the long run, be economically justified.

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